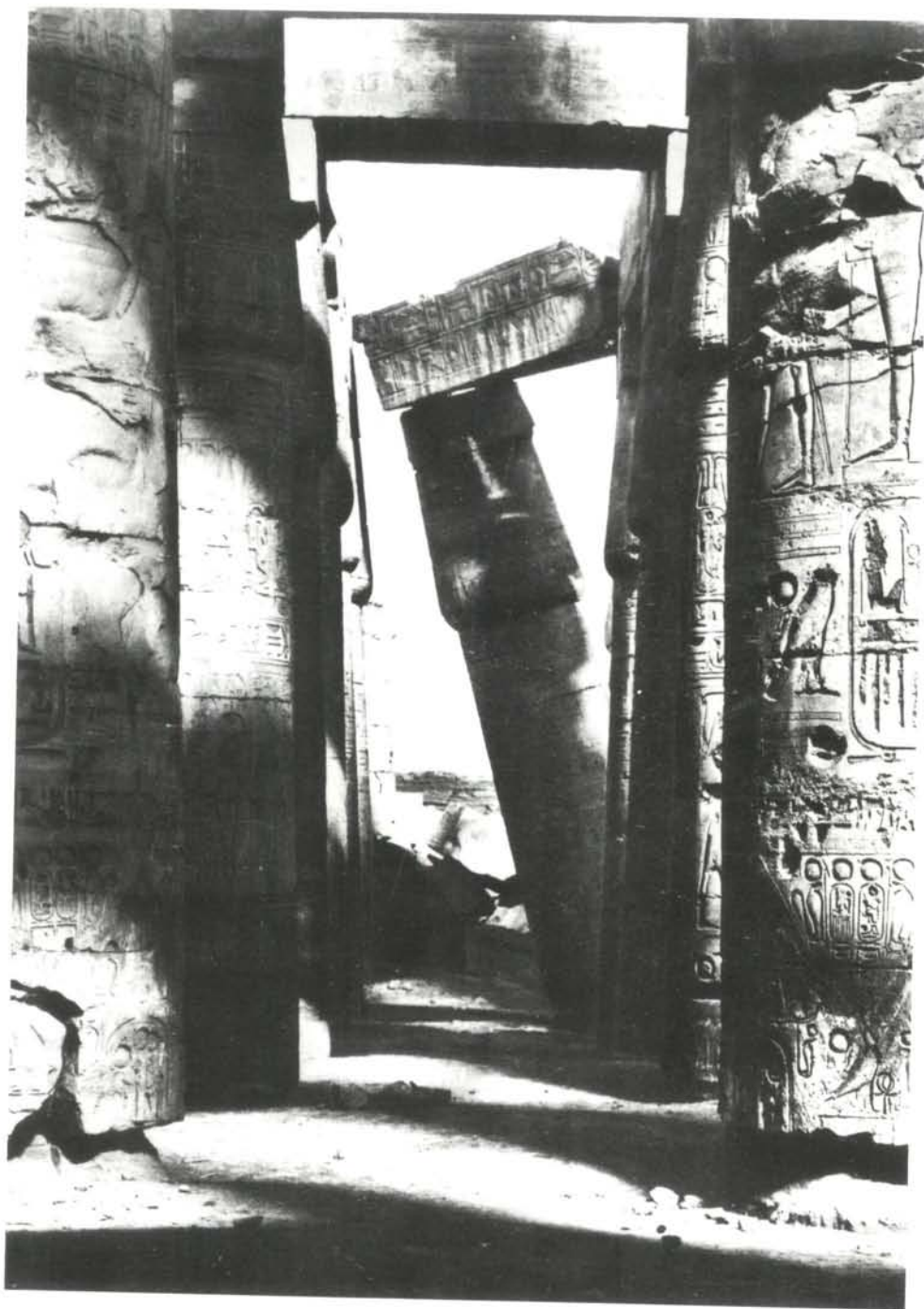


VOLUME 94 • NUMBER 3 • JUNE 1989

The American Historical Review

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Perspectives
 Vol. 20, No. 8 November 1988
 American Historical Association

**Annual Meeting Highlights
 1988 Cincinnati, OH**

The theme for the 1988 meeting is "History in Inquiry and Practice," and the Program Committee has put together an impressive array of sessions that emphasize the many dimensions of the discipline. The meeting will open, with a special literary session, on Tuesday, December 27 entitled, *Perspectives on History: This session will bring together Theodore Hammer, Gertrude Himmelfarb, Lawrence Levine, and Joan Thelen.* This session promises to be a lively and useful one for all historians, and a focus for what promises to be a lively and useful one for all historians, and a focus for what promises to be a lively and useful one for all historians.

**Exhibit Review
 Culture and Comfort:
 People, Parlors, and
 Upholstery, 1850-1930**



Part of the "Culture and Comfort" exhibition, this brilliantly related East and West manufacturing in the United States in 1930. It is a history in its own right, and the upholstery is beautiful.

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In This Issue

Readers may want to turn first to the end of the article section and read the forum titled "The Old History and the New." It includes, in slightly revised form, the papers read by **Theodore S. Hamerow**, **Gertrude Himmelfarb**, **Lawrence W. Levine**, **Joan Wallach Scott**, and **John E. Toews** at the opening session of the most recent Annual Meeting of the American Historical Association. These scholars exchange views on many of the issues currently in dispute in our profession, including the meaning of professionalization itself, the proper objects of historical study, and the problems that critical theory has posed for grounding the truth value of historical statements. Readers may then turn to the contributions authored by **David Harlan**, **David Hollinger**, and **Allan Megill**, which pursue some of the same questions in greater depth. The first book review in this issue also adds to the discussion by continuing the exchange between Gertrude Himmelfarb and Joan Scott.

The initial forum returns to issues discussed in last year's June issue of the *AHR* about intellectual history after the linguistic turn. **David Harlan** enunciates a "textualist" position in arguing that recent developments in literary criticism and the philosophy of language have discredited some of the assumptions on which historical study is grounded, in particular the belief that historians can situate ideas, texts, and people in a historical context. Harlan believes that, by abandoning the futile task of reconstructing a historical context for texts and their authors, intellectual historians can return to what Harlan describes as an earlier understanding of their discipline: intellectual history as a conversation with the dead about things we value.

David Hollinger defends the "contextualist" position. He points out that Harlan treats methodological writings about intellectual history as an autonomous discourse rather than a sustained commentary on practice. Hollinger takes exception to what he characterizes as Harlan's belief that purely philosophical arguments are by themselves sufficient to dismiss as an impossibility the project of studying ideas in their historical context, and he urges that Harlan's accusations against context-oriented historians be translated into discussions of concrete examples of historical writing. Finally, Hollinger is suspicious of what he sees as Harlan's uncritical acceptance of the epistemological implications of poststructuralism and proposes that historians confront the successive fashions of literary theory thoughtfully but without rushing to judgment.

Allan Megill asserts the importance of "description" in historical writing and contends that it is not, as many historians and theorists assume, a trivial preliminary to the now usually privileged task of explanation. He analyzes the proposition advanced by François Furet and others that narrative follows a *post hoc, ergo propter hoc* logic and points out that this notion, with its hidden presupposition that explanation alone is worthy of attention, is mistaken. Following a discussion of the sense in which Fernand Braudel's masterwork, *The Mediterranean and the Mediterranean World*, is an example of narrative history, Megill argues that the distinctions usually made between narrative history, on the one hand, and problem-oriented, structural, or analytical history, on the other, are inadequate to describe what goes on in history writing. He concludes that an adequate theory of historiography would take account of four historiographical tasks—description, explanation, justification, and interpretation—without granting, a priori, privilege to any one task over the others.

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AHR Forum
Intellectual History and the Return of Literature

DAVID HARLAN

THERE WAS A TIME WHEN HISTORIANS thought they had escaped the “merely literary,” when they thought they had established historical studies on the solid foundation of objective method and rational argument. But recent developments in literary criticism and the philosophy of language have undermined that confidence. Now, after a hundred-year absence, literature has returned to history, unfurling her circus silks of metaphor and allegory, misprision and aporia, trace and sign, demanding that historians accept her mocking presence right at the heart of what they had once insisted was their own autonomous and truly scientific discipline.¹

The return of literature has plunged historical studies into an extended epistemological crisis. It has questioned our belief in a fixed and determinable past, compromised the possibility of historical representation, and undermined our ability to locate ourselves in time. The result of all this has been to reduce historical knowledge to a tissue of remnants and fabrications concealing, it is said, an essential absence. This essay describes literature’s return to history, examines the responses of some leading historians, and suggests where we might go from here.

THE RETURN OF LITERATURE was prepared, in the first instance, by Ferdinand Saussure, with his insistence that language constitutes and articulates experience rather than reflecting or expressing it. After Saussure, meaning became a function of the linguistic system, with its fixed rules and paired oppositions, rather than something waiting to be discovered in nature or the past. Poststruc-

This article is for John Patrick Diggins. I am not sure that he agrees with the arguments I have developed here, but all of the questions are his. I would also like to thank S. A. M. Adshead, George Cotkin, Robert Fitzsimmons, Allan Megill, and Leonard Wilcox for taking the time to read this paper, for helping me improve it, and for saving me from innumerable blunders.

¹ The classic description of language’s return is Jacques Derrida’s *Of Grammatology*. As Derrida remarked in the opening chapter, “never as much as at present has [language] invaded, *as such*, the global horizon of the most diverse researches and the most heterogeneous discourses, diverse and heterogeneous in their intention, method, and ideology”; *Of Grammatology*, trans. Gayatri Chakravorty Spivak (Baltimore, Md., 1974, 1976), 6. For a provocative discussion of literature’s return specifically to history, see Linda Orr, “The Revenge of Literature: A History of History,” *New Literary History*, 18 (Autumn 1986): 1–22.

turalism appeared in the late 1960s as an attempt to replace Saussure's grounded, stable, and closed linguistic system with an ungrounded, open, and protean conception of language. The poststructuralists first attacked Saussure's understanding of the sign as the union of a word (the signifier) and the idea or object for which it stands (the signified).² For Jacques Derrida, Roland Barthes, and others, this presumed unity is a fiction: signifiers are not bonded to signifieds; they merely point to other signifiers. Instead of a structured system generating fixed meanings, we were left with an endless chain of signifiers in which meaning is always deferred and finally absent. There is no external point of reference, no ultimate word, no "transcendental signified" that will ground meaning, guaranteeing it once and for all. There is only the incessant and unremitting play of signifiers—signifiers freed from the tyranny of the signified, signifiers no longer chained to the structuralist grid of rules and oppositions. Unbound, ungrounded, relieved of their referential burdens, words became protean and uncontrollable. As Derrida explained, "this, strictly speaking, amounts to destroying the concept of the 'sign' and its entire logic."³

If sign and reference have been the first casualties of poststructuralism, narrative has been the second.⁴ Historians had always assumed that a narrative, especially a historical narrative, contained a fixed and determinable meaning: the writer's views on this or that topic, an expression of personality or character, some representation of the world in which the writer lived, and so on. They usually tried to grasp that meaning by "feeling themselves into" the narrative, trying to experience in it all the concreteness and particularity of the author, idea, or period it was thought to represent. But this traditional notion of narrative has been fatally compromised by the signifiers' escape from their controlling signifieds and from the underlying system of oppositions. For the result of that escape is that words have become protean and prolific. In Donald Barthelme's story "A Picture History of the War," the general cries, "There are worms in words. The worms in words are like Mexican jumping beans, agitated by the warmth of the mouth!"⁵ Because narratives are made out of words, they too begin to generate multiple readings and divergent meanings. What had once been consistent, unified, and self-enclosed suddenly becomes "diversified, mul-

² Derrida's critique of Ferdinand Saussure and structuralist linguistics can be found in his "Structure, Sign and Play in the Discourse of the Human Sciences," in Jacques Derrida, *Writing and Difference* (Chicago, 1978), 278–93; and in Derrida, *Of Grammatology*, 27–73.

³ Derrida, *Of Grammatology*, 7. I have exaggerated the structuralist/poststructuralist opposition in order to point out the crucial differences between them. In actual fact, the possible escape of the signifiers, their tendency to "take off and lead a life of their own," has worried scholars at least since the Renaissance. See Adena Rosmarin, "On the Theory of 'Against Theory,'" *Critical Inquiry*, 8 (Summer 1982): 778–79.

⁴ See Hayden White, *The Content of the Form: Narrative Discourse and Historical Representation* (Baltimore, Md., 1987); Paul Ricoeur, *Time and Narrative*, 3 vols. (Chicago, 1984–88); with J. T. Mitchell, ed., *On Narrative* (Chicago, 1981), an important collection of articles that originally appeared in *Critical Inquiry*; and Wallace Martin, *Recent Theories of Narrative* (Ithaca, N.Y., 1987).

⁵ Donald Barthelme, "A Picture History of the War," in Barthelme, *Unspeakeable Practices, Unnatural Acts* (New York, 1968), 142. Earlier in this same story, Barthelme has the exasperated general beseech no one in particular, "Why does language subvert me, subvert my seniority, my medals, my oldness, whenever it gets a chance? What does language have against me—me that has been good to it, respecting its little peculiarities and niciliosities, for sixty years?" *ibid.*, 139–40.

tifold, and full of contradictions." The coherent narrative, bearing a single, determinate, discoverable meaning, has been "dispersed into clouds of linguistic particles . . . each with its own pragmatic valence."⁶

Historians are a skeptical lot. They tend to feel that one should trust one's nose, like a hunting dog. They are afraid that if they once let themselves be distracted by theory they will spend their days wandering in a cognitive labyrinth from which they will find no way to depart. Literary criticism is clearly the worst of these labyrinths, especially its postmodern version.⁷ Historians avert their eyes, but what little they hear confirms their worst fears: literary theory is esoteric, subversive, anarchistic—something one should avoid as a matter of intellectual hygiene. As the editor of *Critical Inquiry* recently explained, persons of common sense typically regard literary theory as "professionally disreputable, politically ineffectual, morally nihilistic, cognitively inconsequential, stylistically hideous, and intellectually dangerous. It is perceived as a foreign invention (mainly French), a passing fashion, a too-easily domesticated bag of tricks, and an inexplicable temptation for the young."⁸

Nevertheless, postmodern literary criticism has become so powerful and influential across such a broad range of disciplines,⁹ and it has raised so many disturbing questions about the conceptual foundations of history itself, that historians can no longer ignore it. Especially intellectual historians. As the discipline responsible for keeping our cultural memory alive and our intellectual traditions relevant, it is intellectual history that stands to lose the most from the postmodern analysis of representation and narrative.

THE MOST INFLUENTIAL RECENT ATTEMPT TO RECONSTRUCT intellectual history has been made by Quentin Skinner and J. G. A. Pocock. Skinner has written several important studies of early modern political thought and a series of methodological essays. These writings have been widely influential, his critics generally ineffectual, and his attempts to reform intellectual history apparently successful, so much so that perhaps we should grant him his claim to have established "a

⁶ Jean-François Lyotard, *The Postmodern Condition* (Minneapolis, Minn., 1984), xxiv.

⁷ For a brief but interesting discussion of why linguistic skepticism cuts so much deeper than ideological skepticism, and why historians feel so uneasy before "the language thing," see Hans Kellner, "A Bedrock of Order: Hayden White's Linguistic Humanism," *History and Theory*, 19 (1980): 12 and following.

⁸ Tom Mitchell, "The Golden Age of Criticism: Seven Theses," a paper read at the University of Canterbury, July 1987. Richard Rorty attributed the remark about intellectual hygiene to "a distinguished analytic philosopher"; Rorty, *Consequences of Pragmatism: Essays, 1972–1980* (Brighton, 1982), 224.

⁹ Here is J. Hillis Miller describing the contemporary "triumph" of literary theory: "Theory is so dominant everywhere now in teaching and writing in the humanities and social sciences that it would be better to speak of the universal triumph of theory rather than of any effective resistance to it. This is true in spite of the denunciations that continue to appear, for example in the mass media . . . What do I mean by the 'triumph of theory'? I mean what is obvious on all sides, not just the attention now paid to literary theory even in the mass media, but the immense proliferation of essays, books, dissertations, new series by publishers, new journals, courses, curricula, programmes, professorships, conferences, symposia, study groups, institutes, centers, and 'focused research units,' all devoted to critical theory. It is a gigantic paraphernalia of collective intellectual effort"; Miller, "But Are Things as We Think They Are?" *Times Literary Supplement*, 4410 (October 9–15, 1987): 1104.

new orthodoxy” among historians of ideas.¹⁰ That new orthodoxy has been erected on two pillars. The first is Skinner’s recognition that recent developments in the philosophy of language and the philosophy of science—especially the work of Willard Quine, Thomas Kuhn, Paul Feyerabend, and others—have undermined the possibility of building a structure of empirical knowledge on any basis purporting to be independent of or prior to interpretation. The result has been a wholesale retreat from empiricism—a retreat that has occurred in virtually every one of the human sciences.¹¹

The second pillar is an essentially Romantic hermeneutics.¹² From its origins in Greek mythology through its refinement in nineteenth-century biblical scholarship to its emergence as an academic specialty, the guiding objective of Romantic hermeneutics has remained constant: the recovery of authorial intention. Skinner’s appropriation of hermeneutics has led him to insist that the historian’s first responsibility is to recover the author’s “primary intentions,” wherein the real message of the text will be found.¹³

In order to recover the author’s intention, historians have to reconstruct the mental world in which the author wrote his or her book—the entire set of linguistic principles, symbolic conventions, and ideological assumptions in which the author lived and thought. Only by fixing the author’s text in this elaborately reconstructed context can historians hope to recover “all that can have been intended.” In “Hermeneutics and the Role of History,” Skinner condensed his prescriptions into a single syllogism:

1. We need to recover an author’s intentions in writing in order to understand the meaning of what he writes.
2. In order to recover such intentions, it is . . . essential to surround the given text with an appropriate context of assumptions and conventions from which the author’s exact intended meaning can then be decoded.

¹⁰ Quentin Skinner, “Hermeneutics and the Role of History,” *New Literary History*, 7 (1975–76): 214.

¹¹ Quentin Skinner, ed., *The Return of Grand Theory in the Human Sciences* (Cambridge, 1985), 1–20.

¹² “There can be no doubt that the influence of the hermeneutic tradition has in general played a clarifying role in helping to propagate the idea of interpretation as essentially a matter of recovering and rendering the meaning of a text”; Skinner, “Hermeneutics and the Role of History,” 214.

¹³ Skinner regarded “knowledge of such intentions” as “indispensable”; “Hermeneutics and the Role of History,” 211. This is what Martin Jay has referred to as “the illusion that texts are merely congealed intentionalities waiting to be reexperienced at a later date”; Jay, “Should Intellectual History Take a Linguistic Turn? Reflections on the Habermas-Gadamer Debate,” in Dominick LaCapra and Steven Kaplan, eds., *Modern European Intellectual History: Reappraisals and New Perspectives* (Ithaca, N.Y., 1982), 106. This approach to texts first came under attack by the New Critics in the 1930s and 1940s. For a recent defense, see Steven Knapp and Walter Benn Michaels, “Against Theory,” *Critical Inquiry*, 8 (Summer 1982): 723–42; the critical responses in vol. 9 (June 1983) and vol. 11 (March 1985); and Steven Knapp and Walter Benn Michaels, “Against Theory 2: Hermeneutics and Deconstruction,” *Critical Inquiry*, 14 (Autumn 1987): 49–68. Leo Strauss’s followers, of course, have never given up the hope of recovering authorial intention. See Gordon Wood’s discussion of recent Straussian efforts to recover the “original intention” of the Framers in “The Fundamentalists and the Constitution,” *New York Review of Books*, 25 (February 18, 1988): 33–40.

3. This yields the crucial conclusion that a knowledge of these assumptions and conventions must be essential to understanding the meaning of the text.¹⁴

Skinner's prescriptions may have become the "new orthodoxy" among intellectual historians, but, as every churchman knows, orthodoxies generate heretics. In this case, the apostates seem to be "the Yale formalists and their various philosophical allies," by whom Skinner means the poststructuralists. And they seem to be gaining adherents among Skinner's former allies, "a number of recent hermeneutic theorists" openly exhibiting "a curious tendency" to adopt "the formalists' assumptions."¹⁵ In "Hermeneutics and the Role of History," Skinner attacked the poststructuralists directly, condemned their "crude analysis," and dismissed them as "confused."

What Skinner dislikes about the poststructuralists, of course, is their theory of language. For Derrida, Michel Foucault, Paul de Man, and others, language is an autonomous system that constitutes rather than reflects; it is a play of unintended self-transformations and unrestrained self-advertisements rather than a set of stable meanings and external references.¹⁶ And it is intertextual rather than intersubjective, writing its own accumulated meanings over the author's desires and intentions. The paradigm of language for the poststructuralists is therefore not speaking but writing, with its absent author, its unknown audience, and its unruly text spewing out its manifold significations, connotations, and implications.

This understanding of language has at least two immediate implications for intellectual history. First, it suggests that the desiring, thinking, intending subject of that discipline—the author of the classic texts in, for example, political theory—has disappeared, his or her biography reduced to no more than another text, the authority for which lies in still another text, the authority for that lying in yet another text, and so on, ad infinitum. Hence "the death of the author."¹⁷ Second, if the author has disappeared, so has the text; as a discrete, autonomous entity with a determinate and discernible meaning, it too has been dissolved by intertextuality. For the poststructuralist, the text remains significant precisely because it eclipses and transcends its author's intentions.¹⁸

¹⁴ Skinner, "Hermeneutics and the Role of History," 216.

¹⁵ Skinner, "Hermeneutics and the Role of History," 213.

¹⁶ I do not mean to suggest that there are no significant differences between Derrida, Foucault, and de Man concerning the nature of textuality. For a discussion of the differences between Derrida and Foucault on this point, see Edward Said, "The Problem of Textuality: Two Exemplary Positions," *Critical Inquiry*, 4 (1978): 673–714. Paul de Man was especially concerned to preserve something of language's referential capacity; see de Man, *Allegories of Reading: Figural Language in Rousseau, Nietzsche, Rilke, and Proust* (New Haven, Conn., 1970), for his effort to identify the limits of deconstruction. The chapters on Rousseau are especially important in this regard.

¹⁷ See Roland Barthes, "The Death of the Author," in Stephen Heath, ed., *Image-Music-Text: Roland Barthes* (London, 1977). "The names of authors or of doctrines have here no substantial value. They indicate neither identities nor causes. It would be frivolous to think that 'Descartes,' 'Leibniz,' 'Rousseau,' 'Hegel,' etc., are names of authors, of the authors of movements or displacements that we thus designate. The indicative value that I attribute to them is first the name of a problem"; Derrida, *Of Grammatology*, 99.

¹⁸ And because it eclipses and transcends the discourse in which it was written. Derrida has argued that "the text constantly goes beyond this representation [the historian's representation of the text's 'proper' discourse] by the entire system of its resources and its own laws." Moreover, "the

But Skinner believes he can escape all this by thinking of language as speech rather than writing, that is, by utilizing speech act theory.¹⁹ Speech act theory asserts, first, that speech is the paradigmatic model for all language use; second, that speech acts are intersubjective rather than intertextual; third, that speech acts are social acts, that they occur in concrete social situations from which they derive their meaning; fourth, that speech acts are acts in which speakers purposefully manipulate language in order to perform certain actions—they command, they assert, they promise. In other words, speech acts (and by extension, all sentences) are intentional human actions that occur in specific social situations.

If speech act theory could be applied to intellectual history, as Skinner believes, it just might provide the conceptual ground from which our traditional understanding of history could be defended against the onslaught of poststructuralist criticism. Speech act theory might defend intellectual history in four different ways. First, the importance speech act theory attaches to the context in which utterances are made means that texts and their meanings would be anchored in the bedrock of specific historical situations; this would put an end to free-floating signifiers and all the uncertainty they have brought with them. Second, language would resume its formerly transparent nature and once again make itself available for the historian to gaze through. Just as we were accustomed to doing before the poststructuralists arrived, we could read a historical text and peer through its language as if staring through a window, discovering all sorts of things about the author and the world in which the author lived, almost as if we had become one of God's spies. Third, because speech act theory focuses on the intentional subjects doing something (speaking or writing)—because it is intersubjective rather than intertextual—it promises to rescue authors from the oblivion to which poststructuralism has consigned them. Fourth, and stemming directly from this, if speech act theory could be applied to intellectual history, it would reinstall authorial intention as historians' primary concern. But would it work?

The answer is no. Basically, speech act theory is about speaking rather than writing. Speech acts are events located at specific points in time and in concrete socio-cultural contexts. Speaker and hearer are immediately present to each other and share a common reality to which signifiers can be instantly referred and embedded. For these reasons, reference in speech acts is thought to be unproblematic.²⁰ Skinner assumes that the same is true of writing, that writing

question of [the text's] genealogy exceeds by far the possibilities that are at present given for its elaboration"; Derrida, *Of Grammatology*, 101.

¹⁹ As does Pocock. See Pocock, "An Appeal from the New to the Old Whigs? A Note on Joyce Appleby's 'Ideology and the History of Political Thought,'" *Intellectual History Newsletter*, 3 (1981): 47. The classic works of speech act theory are J. L. Austin, *How to Do Things with Words* (Cambridge, Mass., 1963); and John Searle, *Speech Acts: An Essay in the Philosophy of Language* (London, 1972).

²⁰ But it is precisely this "phonocentrism," this privileging of speech over writing, that Derrida has attacked so vociferously. Derrida wants to reduce speech to a form of writing, an "*archi-écriture*." He wants to formulate "a new situation for speech," to enforce "its subordination within a structure of which it will no longer be the archon." Indeed, in *Of Grammatology*, he even declared "the death of speech" (8). His most explicit attack on Austin and speech act theory was in "Signature Event

can be thought of as analogous to speaking, that writing merely transcribes speech into script, or that writing *is* speech, a sort of frozen speech, speech fixed in script. But this is clearly not the case: speech is not the same as script, writers are not the same as speakers, and readers are not the same as listeners; the writer's reader is not analogous to the speaker's hearer. There is no dialogue between writer and reader: the reader does not interrogate the writer, and the writer does not respond to the reader. As Paul Ricoeur explained in *Hermeneutics and the Human Sciences*, "the reader is absent from the act of writing; the writer is absent from the act of reading. The text thus produces a double eclipse of reader and writer."²¹ And if the common reality shared by speaker and hearer cannot be transferred to writer and reader, and signifiers therefore cannot be grounded by the act of pointing (as they are thought to be in speech acts), then reference and representation become highly problematic. Once the text has been liberated from authorial reference, it has also been liberated from authorial intention. So the author vanishes, his or her intentions disappear, and the text begins to suggest possibilities its author may never have imagined.

It is just at this point that Hans Georg Gadamer's work becomes important for historians. As we saw, the hermeneutics that Skinner appropriated was a basically nineteenth-century Romantic hermeneutics designed for, and sharpened to, a single point: the recovery of authorial intention. It required that historians approach the text with minds as open and free from prejudice as possible and try to understand the text on *its* terms rather than in the terms peculiar to their own situations. In other words, it required that historians transport themselves into the culture and into the mind of the author. In *Truth and Method*, Gadamer offered a devastating critique of this entire project.²² Gadamer pointed out first that historians cannot strip themselves of their inherited prejudices and preconceptions in order to project themselves into the minds of their authors because the historians' preconceptions and prejudices are what make understanding possible in the first place. These preconceptions and prejudices are not merely obstacles to be overcome or discarded; they constitute the preconditions for understanding, even though they simultaneously limit its potential achievement. Historians are embedded in their own historical traditions; their understanding of a particular document is made possible by (and circumscribed by) their position in that tradition. "History does not belong to us," Gadamer wrote; "we belong to it." "The self-awareness of the individual is only a flickering in the closed circuits of historical life. That is why the prejudices

Context." *Glyph*, 1 (1977): 172–97. But see John Searle's defense in "Reiterating the Differences," *Glyph*, 1 (1977): 198–208; and Derrida's rejoinder ("Limited Inc abc," *Glyph*, 2 [1977]: 162–254.) This debate has aroused a great deal of interest. Accounts can be found in Gayatri Chakravorty Spivak, "Revolutions That as Yet Have No Model," *Diacritics*, 10 (1980): 29–49; and Christopher Norris, *Deconstruction: Theory and Practice* (London, 1982), 108–15.

²¹ Paul Ricoeur, *Hermeneutics and the Human Sciences* (Cambridge, 1981), 146. In Derrida's words, "writing is the name of these two absences"; *Of Grammatology*, 40–41.

²² For a humorous but stinging critique of this project, see Jorge Luis Borges's story "Pierre Menard, Author of *Don Quixote*," in *Borges, Fictions* (New York, 1962), 42–51.

of the individual . . . constitute the historical reality of his being.”²³ We approach the past, then, not in a state of historical virginity but with all the presuppositions, assumptions, and prejudices that both make us real people located within a particular historical tradition and that make it possible to imaginatively approach some other time.²⁴

Gadamer’s second point is that the text to be interpreted is also embedded in a particular historical tradition—not the tradition in which the text was written (we can never recover that) but the tradition of interpretation that has grown up around the text since it was written.²⁵

Skinner, by contrast, argued that we can strip the text of its accumulated meanings, reconstruct the historical situation in which it was initially written, reinsert the text in its reconstructed context, and there discern its indigenous, prenatal meaning. He wanted to “repristinate” the text. But Gadamer’s analysis shows this to be impossible; the text can never be severed from the interpretations through which it has been passed down to us, interpretations that now “constitute the historical reality of its being.” Understanding a text means understanding its effective history. To pretend otherwise is to transform the text, which “has grown historically and has been transmitted historically,” into “an object of physics.”²⁶ Gadamer can be quite scathing about this:

the reconstruction of the original circumstances, like all such restoration, is a pointless undertaking in view of the historicity of our being. What is reconstructed, a life brought back from the lost past, is not the original. In its continuance in an estranged state it acquires only a secondary, cultural existence . . . Even the painting taken from the museum and replaced in the church, or the building restored to its original condition are not what they once were—they become simply tourist attractions. Similarly, a hermeneutics that regarded understanding as the reconstruction of the original would be no more than the recovery of a dead meaning.²⁷

Gadamer’s critique has been the spearhead of what Skinner now acknowledges is a “growing refusal . . . to treat the recovery of an intended meaning as any part of the interpreter’s task.”²⁸ In the middle of the 1980s, Skinner found himself called upon to defend a position that only ten years ago he had triumphantly dubbed “the emerging orthodoxy.” As one might expect, his recent declarations have had an increasingly defensive tone about them; he

²³ Hans Georg Gadamer, *Truth and Method* (New York, 1975), 245.

²⁴ Gadamer recognized that identifying “legitimate” prejudices is one of hermeneutics’ “fundamental” problems. Gadamer, “Prejudices as Conditions of Understanding,” which comprises Part Two of *Truth and Method*. See the first part of this section, “The Rehabilitation of Authority and Tradition,” 245–53.

²⁵ Gadamer referred to this as “its constantly renewed reality of being experienced”; *Truth and Method*, xix. Gadamer’s position, as I have outlined it here, is in some ways an extension of Martin Heidegger’s insistence on historicity of the text, his insistence that both our texts and our understandings of them are irremediably historical. See Richard Rorty’s discussion of Heidegger in “Overcoming the Tradition: Heidegger and Dewey,” *Consequences of Pragmatism: Essays, 1972–1980* (Brighton, 1982), 37–59.

²⁶ Rorty, *Consequences of Pragmatism*, xxi.

²⁷ Rorty, *Consequences of Pragmatism*, 149. David Hoy has made the same point: meaning is not “a given, an in-itself, which only needs to be unfolded” but is rather “conditioned by its history of reception and influence”; Hoy, *The Critical Circle* (Berkeley, Calif., 1978), 103 and 93.

²⁸ Skinner, *Return of Grand Theory to the Human Sciences*, 8.

sounds besieged, as if he had barricaded himself in his office, writing from behind upturned filing cabinets. He charges that the "proliferating philosophical doubts" and "moral objections that have been raised of recent years" have been planted by "all-purpose subversives" who are "seeking to demolish the claims of theory and method to organize the materials of experience." And he warns that these "threats to the foundations of the human sciences" confront us with nothing less than "the spectre of epistemological relativism."²⁹

THE CRITICAL SITUATION IN WHICH INTELLECTUAL HISTORY now finds itself is even more apparent in J. G. A. Pocock's recent work. Like Skinner, he has made major contributions to both the practice and the theory of history. Also like Skinner, he began his career as part of the Cambridge University group that included Peter Laslett, and John Dunn. In a series of brilliant books and articles that started appearing in the mid-1960s, the Cambridge group argued that historians should pay more attention to "the changing function, context and application of conceptual languages . . . found in particular societies at particular times."³⁰ Once the conceptual languages of a particular society had been recovered and described, historians would then have access to the menu of meanings that those languages made available (or denied) to writers and readers living in that culture. This was an enormously powerful idea that allowed the Cambridge historians to rewrite huge sections of the history of British political thought. By analyzing the conceptual language of seventeenth-century Britain, they were able to show, for example, that the contemporary English landowning class could not possibly have seen the revolutionary implications that later generations read into John Locke's *Second Treatise*. In other words, by studying the conceptual language of a particular culture, we could learn what it was or was not possible for people in that culture to have thought. For, as Pocock has commented, "Men cannot do what they have no means of saying they have done; and what they do must in part be what they can say and conceive that it is."³¹ The historian tries "to point out conventions and regularities that indicate what could and could not be spoken in the language, and in what ways the language *qua* paradigm encouraged, obliged, or forbade its users to speak and think."³² And,

²⁹ Skinner, *Return of Grand Theory to the Human Sciences*, 1–20, *passim*. One hears historians voicing this sort of alarmist reaction more and more often these days. James Kloppenberg recently warned his colleagues that developments in literary criticism threaten to "bring all critical exchange to an end," that meaning itself—"all meanings"—will shortly "collapse into unintelligibility." Contemporary literary theory simply "makes writing history impossible"; Kloppenberg, "Deconstruction and Hermeneutic Strategies for Intellectual History," *Intellectual History Newsletter*, 9 (April 1987): 7, 10. Stanley Fish discussed such "theory fear" in "Consequences," *Critical Inquiry*, 11 (1985): 439 and following. For a suggestion that "theory fear" may be widespread among historians, see Dominick LaCapra, "On Grubbing in My Personal Archives: An Historiographical Exposé . . ." *Boundary 2: A Journal of Postmodern Literature and Culture*, 13 (Winter–Spring 1985): 43–68.

³⁰ Pocock as quoted by Joyce Appleby, "Ideology and the History of Political Thought," *Intellectual History Group Newsletter*, 2 (1980): 11.

³¹ Pocock, as quoted by Appleby, *ibid.*, 15.

³² J. G. A. Pocock, *Virtue, Commerce, and History: Essays on Political Thought and History, Chiefly in the Eighteenth Century* (New York, 1985), 10.

from that, what writers living in a particular culture could (and could not) have intended by the words they wrote. And what readers (for instance, Locke's readers) could or could not have understood by their words.

Skinner had wanted to turn historians toward the study of language as a means of recovering authorial intention, but Pocock had hoped to interest them in language itself, especially in the evolution of particular language systems over long periods of time. In this, he has been enormously successful. As early as 1971, he was able to describe and celebrate "the emergence of a truly autonomous method, one which offers a means of treating the phenomena of political thought strictly as historical phenomena."³³ Indeed, on both sides of the Atlantic, intellectual historians now occupy themselves, as Pocock had hoped they would, with "the investigation of entire political languages, including the asking of how they interact with and gain predominance over one another."³⁴

Like Fernand Braudel, François Furet, Emmanuel Le Roy Ladurie, and other members of the *Annales* school, Pocock emphasized *la longue durée*, the enduring continuities of thought and perception through long, sweeping movements of time. In *The Machiavellian Moment*, for example, he described how the language of civic humanism had evolved over a period of five hundred years, from fifteenth-century Italy to nineteenth-century America. But here we come upon the central problem of Pocock's approach: the focus on languages or discourses as they evolve, expand, contract, and displace one another over long stretches of time tends to obscure the contributions of individual thinkers, certainly not the intention of the Cambridge historians. But, even in their own field of the history of British political thought, their work sent Thomas Hobbes, Locke, and the other intellectual giants of the British past into a "conceptual limbo."³⁵ Viewed from the perspective of linguistic systems gradually evolving over hundreds of years, individuals—be they French peasants or British writers—simply disappear under the wheel of time.

No one knows this better than Michel Foucault, the acknowledged master of the history of discourses until his death in 1984. Read twenty medical texts written between 1770 and 1780, Foucault suggested, then read another twenty written between 1820 and 1830. "In the space of forty or fifty years everything has changed: what one talked about, the way one talked about it; not just remedies, of course, not just the maladies and their classifications, but the outlook itself. Who was responsible for that? Who was the author of it?" An entire conceptual world has disappeared, its place taken by a new "discourse," a new grid with its own exclusions and erasures, "a new play with its own rules, decisions and limitations, with its own inner logic, its own parameters and blind alleys."³⁶ In other words, the rules governing the formation and transformation

³³ Pocock, *Politics, Language and Time*, 3, 11. Pocock modestly conceded that he himself "seems to have been concerned in this transformation from an early stage"; 3.

³⁴ David Hollinger, "Historians and the Discourse of Intellectuals," in John Higham and Paul K. Conkin, eds., *New Directions in American Intellectual History* (Baltimore, Md., 1979), 60 n. 2.

³⁵ Appleby, "Ideology and the History of Political Thought," 15.

³⁶ Michel Foucault, in Fons Elders, ed., *Reflexive Water: The Basic Concerns of Mankind* (London, 1974), 150.

of any discourse are invoked beneath the writer's awareness. Hence Foucault's decision, announced in the early pages of *The Archaeology of Knowledge*, to "abandon any attempt to see discourse as a phenomenon of expression." "Discourse is not the majestically unfolding manifestation of a thinking, knowing, speaking subject, but, on the contrary, a totality, in which the dispersion of the subject, and his discontinuity with himself may be determined."³⁷

By its very subject matter, by its inevitable preoccupation with the abrupt transformations and sudden disruptions that mark the life of discourses, and by its focus on *la longue durée*, the history of discourses disperses the historical agent, "the knowing subject."³⁸ This is why Foucault speaks so derisively of "what you might call the creativity of individuals"; indeed, this is why individuals have been virtually obliterated from his histories. The transformation of intellectual history into a history of discourse implies a loss that some historians may not want to accept.

Pocock being one of them. In *Virtue, Commerce, and History*, his latest work, he drew a distinction between the "history of political thought" and the "history of political discourse" and acknowledged that the professional current is flowing from the first to the second. But Pocock set himself solidly against that movement. And he did so for a crucial and revealing reason: because he is, in his words, committed to writing history that is "ideologically liberal."³⁹ By which he means history that preserves the integrity of the subject. Unlike the "history of political discourse," the "history of political thought" will remain a history of "men and women thinking."⁴⁰

In *Virtue, Commerce, and History*, Pocock asks us to think of the subject—the writer of political theory in seventeenth-century England, for instance—as a creative agent self-consciously manipulating a "polyvalent" language system. By "polyvalent," Pocock means that the individual words of such a system "denote and are known to denote different things at the same time."⁴¹ Viewed this way, every language system is a melange of "sublanguages, idioms, rhetorics and modes of speech, each of which varies in its degree of autonomy and stability."⁴² The writer stands outside of and before this linguistic jungle, confronts it as a set of verbal possibilities to be manipulated and exploited in order to realize his or her intentions—intentions *brought* to the scene of writing. "The author may move among these patterns of polyvalence, employing and recombining them according to the measure of his capacity." The text produced is not a linguistic

³⁷ Michel Foucault, *The Archaeology of Knowledge and the Discourse of Language*, trans. A. M. Sheridan Smith (New York, 1972), 55. Foucault's perspective is hardly unique, of course; the dispersion of the subject has been a strong element of French intellectual life since the 1960s, most obviously in the work of Claude Lévi-Strauss, Louis Althusser, and the *Annales* historians, among others.

³⁸ As Foucault wrote, "instead of referring back to the synthesis of the unifying function of a subject, the various enunciative modalities manifest his dispersion. To the various statuses, the various sites, the various positions that he can occupy or be given when making a discourse. To the discontinuity of the planes from which he speaks"; *Archaeology of Knowledge*, 54, 149.

³⁹ Pocock, *Virtue, Commerce, and History*, 34.

⁴⁰ Pocock, *Virtue, Commerce, and History*, 1–2.

⁴¹ Pocock, *Virtue, Commerce, and History*, 30, my emphasis.

⁴² Pocock, *Virtue, Commerce, and History*, 7.

heterocosm but the product of “a single powerful mind” and so possesses all “the rhetorical, logical, or methodical unity its author imposed on it.” It is “an articulation of the author’s consciousness.” When historians read it, they therefore enter into “a communication with the author’s Self.”⁴³

But it is precisely this longing for the author’s presence—a presence that seems to shimmer just beneath the surface of the text but a presence that is, in fact, always deferred, always elsewhere, always already absent—it is this yearning for communion with the author’s self that recent developments in literary criticism and the history of discourse have brought powerfully into question.⁴⁴ Barthes, Derrida, Foucault, and others have made us painfully aware of the desires we bring to our texts: our desire to find in them some compensating sense of connection and completeness, some reassurance of fullness and amplitude. It is difficult, after Barthes, Derrida, and Foucault, to continue approaching our texts as objects that *should* be transparent, as signs of something else, as masks concealing something held in reserve, something that will, in the end, be revealed as whole and primary and essential, a perfect presence.

Herman Melville tried to tell us this over a hundred years ago. Is this not the meaning of Ahab’s terrible quest for the white whale? Was Ahab not trying to read the whale as we read our texts, trying to pierce its empty whiteness, to penetrate its maddening blankness in order to reveal some original presence? Is this not what Ahab tried to explain to Starbuck as they stood on the quarter-deck of the Pequod?

Hark ye yet again—the little lower layer. All visible objects, man, are but as pasteboard masks. But in each event—in the living act, the undoubted deed—there, some unknown but still reasoning thing puts forth the mouldings of its features from behind the unreasoning mask. If man will strike, through the mask! How can the prisoner reach outside except by thrusting through the wall? To me, the white whale is that wall, shoved near to me. . . . that inscrutable thing is chiefly what I hate; and be the white whale agent, or be the white whale principal, I will wreak that hate upon him.⁴⁵

Pocock is well aware of all this, of course, as he is of recent developments in literary criticism and the philosophy of language. He knows, as he himself admits, that languages allow “the definition of political problems and values in certain ways and not in others.” He has acknowledged that intentions cannot exist outside of language, that “the modes of speech available to [a writer] give him the intentions he can have.”⁴⁶ And he knows that the image of a writer standing outside his or her own universe of discourse, anterior to it, as it were, manipulating and exploiting it in order to express intentions the writer has

⁴³ Pocock, *Virtue, Commerce, and History*, 9, 23, 25.

⁴⁴ We did not have to wait for contemporary literary criticism to tell us this; it was precisely the desire to find in the text some communion between writer and reader that Nathaniel Hawthorne warned us against in the preface to *The Scarlet Letter*: “When he casts his leaves forth upon the wind,” Hawthorne wrote, the author imagines himself addressing “the few who will understand him, better than most of his schoolmates and lifemates . . . as if the printed book, thrown at large on the wide world, were certain to find out the divided segment of the writer’s own nature, and complete his circle of existence by bringing him into communion with it”; Hawthorne, *The Scarlet Letter* (1850; rpt. edn., New York, 1970, 1971, 1973), 35.

⁴⁵ Herman Melville, *Moby Dick* (1851; rpt. edn., New York, 1986), 156–57).

⁴⁶ Pocock, *Virtue, Commerce, and History*, 8, 5.

somehow brought to it, will seem highly improbable to literary critics, if not to historians. In other words, he knows that we can no more hope to encounter “the author’s Self”⁴⁷ hiding behind the text than Ahab could hope to confront God behind “the unreasoning mask” of the white whale. But Ahab did hope, and so does Pocock.

J. G. A. Pocock is the most theoretically sophisticated of practicing historians. By his refusal to abandon the dream of authorial presence, by his insistence that intellectual history is a quest to recover “men and women thinking,” he shows us, more clearly than anyone else, the impasse to which the discipline has been brought.⁴⁸

THE NEW DISCOURSE-ORIENTED HISTORIANS who now dominate the writing of American intellectual history seem happily oblivious of the danger that worries Pocock—that a history of discourse will dissolve the traditional subject of intellectual history, the thinking, creating author of past time—will reduce an author, in Pocock’s words, “to the mere mouthpiece of his own language.”⁴⁹ They simply assume, without much discussion, that they can submerge the subject in a history of discourses yet somehow retain the subject as an intending agent.⁵⁰

But, if Pocock and the discourse historians differ about the dangers facing the subject, they are as one when it comes to the dangers of “presentism,” which they all regard as the scourge of professional historiography. By “presentism,” they mean, in John Dunn’s words, “the weird tendency of much writing, in the history of political thought especially, to be made up of what propositions in what great books remind the author of what propositions in what other great books.”⁵¹ There is nothing new about this charge; Herbert Butterfield used to criticize his colleagues for what he called “the pathetic fallacy,” by which he meant “abstracting things from their historical context and judging them apart from their context—estimating them and organizing the historical study by a system of direct references to the present.”⁵²

⁴⁷ Pocock, *Virtue, Commerce, and History*, 25.

⁴⁸ Hayden White has struggled with this same dilemma: on the one hand, an acute sensitivity to the ways in which language both constitutes and dissolves the subject; on the other hand, a deep commitment to liberal humanism, to the human subject and epistemological freedom. Hans Kellner has described White’s dilemma with admirable deftness: “If language is irreducible, a ‘sacred’ beginning, then human freedom is sacrificed. If men are free to choose their linguistic protocols, then some deeper, prior, force must be posited. White asserts as an existential paradox that men *are* free, and that language *is* irreducible”; Kellner, “A Bedrock of Order,” 23.

⁴⁹ Pocock, *Virtue, Commerce, and History*, 5.

⁵⁰ Not all of the discourse historians are so oblivious. Thomas Haskell, for example, has warned of a “profoundly deterministic” current in much of contemporary intellectual history, although this does not cause him to reconsider the call for a discourse-based history. See Thomas Haskell, “Deterministic Implications of Intellectual History,” in John Higham and Paul K. Conkin, eds., *New Directions in American Intellectual History* (Baltimore, Md., 1979), 145.

⁵¹ John Dunn, *Political Obligation in Its Historical Context* (London, 1980), 15, as quoted by Richard Rorty in Rorty, J. B. Schneewind, and Quentin Skinner, eds., *Philosophy in History: Essays on the Historiography of Philosophy* (New York, 1984).

⁵² As quoted by George Stocking, “On the Limits of ‘Presentism’ and ‘Historicism’ in the Historiography of the Behavioral Sciences,” in Stocking, *Race, Culture, and Evolution: Essays in the*

As this sentence from Butterfield suggests, the other side of “presentism” is “contextualism,” the insistence that a particular text can only be understood by placing it in the “historically and socially specific context of public discussion” in which it was written.⁵³ In other words, in a particular “network of intellectual discourse.” In practice, this is coming to mean the various specialized discourses of intellectuals, a history of “the communities of discourse in which they function, and of the varying relations they manifest toward the larger culture.”⁵⁴

The history of discourse (“radical contextualism” as some of its proponents call it) has become the most prevalent and influential tendency among American intellectual historians—the dominant and now conventional orthodoxy.⁵⁵ *New Directions in American Intellectual History*, the most recent survey of the field, was dominated by the contextualists, as is the *Intellectual History Newsletter*. The contextualists are well placed, well organized, and increasingly intolerant of alternative approaches.⁵⁶

But contextualism faces real problems, problems that are becoming increasingly evident. The most obvious and pressing of them concerns the difficulty of defining the relevant “network of intellectual discourse” with any precision.⁵⁷ Some contextualist-minded historians have insisted that every text must be placed within “the immediate context of lesser people, institutions, and issues in which [its author] actually lived and worked.”⁵⁸ But other contextualists contend that the relevant context may turn out to be less immediate, indeed, that it may turn out to be distressingly remote. In “Historians and the Discourse of Intellectuals,” David Hollinger—one of the leading advocates of contextualism—conceded that the relevant context may have to be defined so that it includes all “the theoretical knowledge, literary and religious traditions, and other cultural resources that historians know to have been accessible to most well-informed members of a given society at a given historical moment.”⁵⁹ And, in *Victorian Anthropology*, George Stocking, a historian of anthropology who has played a prominent role in the debates over contextualism, has developed an approach he

History of Anthropology (New York, 1968), 3. The term “pathetic fallacy” was originally coined by John Ruskin as a derogatory reference for the attribution of human feelings to objects in nature. See “Of the Pathetic Fallacy,” in John Ruskin, *Modern Painters*, vol. 3 (Boston, n.d.), 200–18.

⁵³ Hollinger, “Historians and the Discourse of Intellectuals,” 54.

⁵⁴ Dominick LaCapra, “Rethinking Intellectual History and Reading Texts,” in Dominick LaCapra and Steven Kaplan, eds., *Modern European Intellectual History: Reappraisals and New Perspectives* (Ithaca, N.Y., 1982), 69. But see also the symposium on intellectual history and the history of discourses, *Intellectual History Newsletter*, 1 (Spring 1979), especially the essays by Haskell, Bruce Kuklick, Stocking, Skinner, David Hall, and William R. Taylor.

⁵⁵ Both Hollinger and Haskell use the term “radical contextualism.” See David Hollinger, “T. S. Kuhn’s Theory of Science and Its Implications for History,” *AHR*, 78 (April 1973): 377; and Haskell, “Deterministic Implications of Intellectual History,” 138.

⁵⁶ See LaCapra, “Grubbing in My Personal Archives”; and the discussion of Diggins’s *Bard of Savagery*, below.

⁵⁷ I have chosen not to deal here with a collateral problem: that the concept of a discourse implies the anterior concept of (and identification of) historical “periods.” For an insightful discussion of this problem, see Fredric Jameson, *The Political Unconscious: Narrative as a Socially Symbolic Act* (London, 1981), 28.

⁵⁸ Thomas Haskell, “Veblen on Capitalism: Intellectual History in and out of Context,” *Reviews in American History*, 7 (1979): 559.

⁵⁹ Hollinger, “Historians and the Discourse of Intellectuals,” 55.

calls “multiple contextualization.”⁶⁰ As one reviewer noted, “So ‘multiple’ are Stocking’s approaches that even to list them would require more words than a review like this one can use . . . He is sensitive also to the social, economic and political pressures that help shape ideas.”⁶¹ Similarly, the historian Dominick LaCapra has identified at least six different kinds of networks in which texts would have to be placed for contextual analysis, each of which “must be understood to encompass not only other contemporaneous writers and readers but also the traditions tapped and even the partly repressed impulses that do not conform to the prevailing conventions of any community.”⁶² In other words, the relevant community of discourse may include all of Western civilization. And more.

Second, before historians can place a text in its putative context, they must (re)constitute that context—itsself a poetic act—and then interpret it, just as if it were itself a text.⁶³ In other words, we can know no “context” that has not already been textualized.⁶⁴ This is not a novel observation. As William James explained nearly three-quarters of a century ago, a context somehow anterior to textualization—a “reality ‘independent’ of human thinking”—is “a thing very hard to find.”

It reduces to the notion of . . . some aboriginal presence in experience, before any belief about the presence had arisen, before any human conception had been applied. It is what is absolutely dumb and evanescent . . . We may glimpse it, but we never grasp it; what we grasp is always some substitute for it which previous human thinking has peptonized and cooked for our consumption . . . We might say that wherever we find it, it has been already *faked*.⁶⁵

The basic distinction between text and context may not have collapsed everywhere, but even among epistemological conservatives it seems to have become a problem.⁶⁶

Finally, contextualism also suffers from its often-complained-of tendency to reduce complex works to the status of documents.⁶⁷ When the discourse-

⁶⁰ See Stocking, “On the Limits of ‘Presentism’ and ‘Historicism’”; his essay in *Intellectual History Newsletter* (referred to in n. 55 above); and the introduction to his book *Victorian Anthropology* (New York, 1987).

⁶¹ See the review by George Levine, *New York Times Book Review*, March 1, 1987.

⁶² LaCapra as paraphrased by Kloppenberg, “Deconstruction,” 18.

⁶³ As LaCapra has pointed out, “the context itself would have to be seen as a text of sorts. Its ‘reading’ and interpretation pose problems as difficult as those posed by the most intricate written text”; LaCapra, *Rethinking Intellectual History: Texts, Contexts, Language* (Ithaca, N.Y., 1983), 116–17.

⁶⁴ “The age already in the *past* is in fact constituted in every respect as a text”; Derrida, *Of Grammatology*, lxxxix, emphasis in the original. Fredric Jameson made the same point in *The Political Unconscious*: the context is not “immediately present as such, [is] not some common-sense external reality . . . but rather must itself always be reconstituted after the fact”; 81.

⁶⁵ James as quoted by Richard Poirier, *The Renewal of Literature: Emersonian Reflections* (New York, 1987), 48, emphasis in the original.

⁶⁶ As John Toews remarked after a discussion of the inevitable textualization of context, “One begins to wonder if it is possible to avoid the pitfalls of referential or representational theory at all without ceasing to ‘do’ history and restricting oneself to thinking about it”; Toews, “Intellectual History after the Linguistic Turn: The Autonomy of Meaning and the Irreducibility of Experience,” *AHR*, 92 (October 1987): 886.

⁶⁷ In addition to LaCapra’s complaints, cited in note 69 (below), see those of Norman Grabo and Patricia Caldwell in Sacvan Bercovitch, ed., *The American Puritan Imagination* (Cambridge, 1974), 26, 33, 36.

oriented historians look at a particular text, they want to know how it functioned within a specific discourse, what (if anything) it contributed to that discourse, how it influenced or changed the discourse, and so on.⁶⁸ In other words, their primary interest lies in the context rather than the text. Interest in the text is purely instrumental: they want to know what it can tell them about the discourse of which it is, to them, but a manifestation, a token, a document.⁶⁹ LaCapra has dubbed this “intellectual history as retrospective symbolic or cultural anthropology”—a discipline in which “complex texts” are systematically diminished by being used as evidence in the reconstruction of one or another historical discourse. In other words, an activity in which texts are, once again, approached as something other than themselves.⁷⁰

Radical contextualism—whether in the shape of Skinner and Pocock’s history of political thought or in the guise of a history of discourse—is one of the most important and influential attempts yet made to halt the decline of intellectual history. It has acknowledged and has tried to incorporate some of the recent developments in literary criticism, the philosophy of language, and the philosophy of science, and it was once—not long ago—widely regarded as the blueprint for what Pocock called a “truly historical method.” But it has not given us an effective response to the criticisms, doubts, and suspicions that the poststructuralists have raised about intellectual history: the belief that language is an autonomous play of unintended transformations rather than a stable set of established references, a wayward economy of oppositions and differences that constitutes rather than reflects; the consequent doubts about language’s referential and representational capacities; the growing suspicion that narrative may be incapable of conveying fixed, determinate, accessible meaning; and, finally, the eclipse of the author as an autonomous, intending subject. For all its interest in language and discourse, radical contextualism has given us neither the means with which to rebut these claims and doubts nor any suggestions for how we might build on them.

SUPPOSE FOR A MOMENT THAT THE LITERARY CLAIMS lodged against intellectual history are true. Suppose that intellectual history does indeed stand naked before its critics, its canon exposed as a pretentious imposition on the past, its

⁶⁸ Hollinger, “Historians and the Discourse of Intellectuals,” 53.

⁶⁹ “There is at present an excessive tendency to give priority to social and sociocultural approaches and to downgrade the importance of reading and interpreting *complex texts*”; LaCapra, “Rethinking Intellectual History,” 83. LaCapra criticized this tendency. Hollinger, on the other hand, wants us to go even further in that direction. He would have us approach complex texts in a thoroughly instrumental fashion, using them to illuminate the discourse of which they are but manifestations. See Hollinger, “Historians and the Discourse of Intellectuals,” 43, 44.

⁷⁰ LaCapra, “Rethinking Intellectual History,” 83. See Hayden White, “The Context in the Text: Method and Ideology in Intellectual History,” in White, *The Content of the Form* (Baltimore, Md., 1987), 185–213, for an interesting discussion of this tendency. One of the ironies of contextualism is that in their attraction to, affinity for, and complicity with scientific rigor, contextualist historians resemble no one so much as their arch-antagonists, the deconstructionist critics. See Eugene Goodheart, *The Skeptical Disposition in Contemporary Criticism* (Princeton, N.J., 1984), 149–54, for an interesting discussion of deconstruction along these lines.

hope of recovering authorial intention seen to be little more than a metaphysical yearning, its traditional texts sacrificed to the insatiable maw of intertextuality. Northrop Frye's description of the New Criticism would fit intellectual history even better: "a mystery religion without a gospel."⁷¹ Where do we go from here?

We can begin with the problem of texts: with which texts should intellectual historians concern themselves? Any attempt to privilege a particular set of texts is bound to seem problematic these days, given the claims of intertextuality, which dissolves the autonomous identity of individual works. The poststructuralists want us to think of individual texts as the products and outgrowth of other texts written before them. Any particular text, we are told, is merely a "recoded" or "transcoded" version of other texts, texts whose antecedents are to be found in still earlier texts. Behind the individual text, there are only other texts that refer (when they are not merely self-referential) to still other texts, and so on, in an apparently infinite regress. Reference becomes intertextual, origin dissolves, and the individual text is dispersed. Along with it goes any meaningful distinction between "great books" and comic books.⁷²

In actual fact, however, no one, even among the poststructuralists, has any trouble telling the "great books" from the comic books.⁷³ Not even Barthes, who first popularized the notion of "intertextuality." Barthes distinguishes "readerly" (*lisible*) texts from "writerly" (*scriptible*) texts. By "readerly" texts, he means texts that comply with accepted conventions of reading and interpreting. Because we "know how" to read them, we read them passively, finding in them exactly the meaning our conventions identify for us. "Writerly" texts, on the other hand, challenge the conventions that isolate and identify meaning in the "readerly" text. In order to find meaning in the "writerly" text, the reader has to enter the text personally, has to participate actively in the fabrication of whatever meaning is to be carried away. In other words, "writerly" texts force the reader, while reading, to also be mentally composing an alternative or "virtual" text. In this way, "writerly" texts "initiate performances of meaning rather than actually formulat[e] meanings themselves."⁷⁴ They arouse, cultivate, and guide the reader's creation of meaning. They invite the reader to rewrite them and so

⁷¹ Northrop Frye as quoted by Jonathan Culler in his foreword to Tzvetan Todorov, *The Poetics of Prose* (Ithaca, N.Y., 1977), 7.

⁷² Intertextuality is most closely associated with Roland Barthes and Julia Kristeva, but the idea is hardly new with them. Jorge Luis Borges, for example, has been playing with the notion for years. See his story "The Circular Ruins," in Borges, *Fictions*, for a good example. For an attempt to save the autonomous text from the maw of intertextuality, see Stanley Fish, *Is There a Text in This Class: The Authority of Interpretive Communities* (Cambridge, Mass., 1980); and Geoffrey H. Hartman, *Saving the Text: Literature/Derrida/Philosophy* (Baltimore, Md., 1981), especially chap. 5, "Words and Wounds."

⁷³ LaCapra makes the same point; see "Rethinking Intellectual History," 51. The problems and disagreements have less to do with identifying canonical works than with deciding how to interpret them. For an interesting discussion of (and response to) recent attacks on the canon in American literary history, see Werner Sollors, "A Critique of Pure Pluralism," in Sacvan Bercovitch, ed., *Reconstructing American Literary History* (Cambridge, Mass., 1986), 250–79.

⁷⁴ Wolfgang Iser as quoted by Jerome Bruner, *Actual Minds, Possible Worlds* (Cambridge, Mass., 1986), 25. Iser stated that this sort of reading is "unique to literature"; Iser, *The Act of Reading: A Theory of Aesthetic Response* (Baltimore, Md., 1978), 109. But nowhere did he explain why it cannot be applied to other narrative forms as well.

seduce the reader into becoming a writer. This is what Barthes meant when he wrote, "the text you write must prove to me *that it desires me*."⁷⁵

LaCapra probably had something like this in mind when he formulated the distinction between "complex works" and "documents." Like Barthes's "writerly" texts, LaCapra's "complex works" are distinguished by their tendency to subvert the accepted protocols and conventions of reading. They carry out "the contestatory function of questioning [received understandings] in a manner that has broader implications for the leading of life."⁷⁶

Barthes's notion of the "writerly" text is also analogous to Frank Kermode's definition of the "canonical" work.⁷⁷ Canonical works are those texts that have gradually revealed themselves to be multi-dimensional and omni-significant, those works that have produced a plenitude of meanings and interpretations, only a small percentage of which make themselves available at any single reading. Canonical texts have "qualities not to be detected save at an appropriate moment in the future."⁷⁸ They generate new ways of seeing old things and new things we have never seen before. No matter how subtly or radically we change our approach to them, they always respond with something new; no matter how many times we reinterpret them, they always have something illuminating to tell us. Their very indeterminacy means that they can never be exhausted. As Wolfgang Iser observed, the canonical text "refuses to be sucked dry and thrown on the rubbish heap."⁷⁹ Canonical works are multi-dimensional, omni-significant, inexhaustible, perpetually new, and, for all these reasons, "permanently valuable." It is possible, then, to distinguish the "great books" from the comic books, intertextuality notwithstanding. By drawing on Barthes, LaCapra, and Kermode, we can patch together provisional, working criteria for identifying the books with which intellectual history might concern itself.⁸⁰

But now another problem arises: is this list of canonical works, arranged in its neat chronological order, the product of a genuine historical process? Does it truly represent the embodied voices of a great conversation that has been carried on across the ages by the intellectual giants of Western history? Have the great thinkers of every age actually been talking to one another in an extended historical conversation, the great books being the textualized remains of that elevated dialogue?

These questions were raised at least as early as the 1930s, when they were

⁷⁵ Roland Barthes, *The Pleasure of the Text*, trans. Richard Miller (New York, 1975), 6, emphasis in the original.

⁷⁶ LaCapra, "Rethinking Intellectual History," 65. But see also LaCapra, *History and Criticism* (Ithaca, N.Y., 1985), 18 and following, 38 and following.

⁷⁷ Frank Kermode, *Forms of Attention* (Chicago, 1985).

⁷⁸ Kermode, *Forms of Attention*, 75.

⁷⁹ Iser, *Act of Reading*, 7. "Instead of being able to grasp meaning like an object, the critic is confronted by an empty space. And this emptiness cannot be filled by a single referential meaning, and any attempt to reduce it in this way leads to nonsense"; 8.

⁸⁰ Hayden White also drew a distinction between the comic books and the "classic texts." The difference "has to do with the extent to which the classic text reveals, indeed actively draws attention to, its own processes of meaning production and makes of these processes its own subject matter, its own 'content.'" *The Education of Henry Adams* is thus a classic text because of its "self-conscious and self-celebrating creativity"; White, *Content of the Form*, 212.

directed against Arthur Lovejoy's "History of Ideas" project. And they were raised again, more aggressively, by the new social historians in the 1960s and in the early methodological essays by Pocock and Skinner.⁸¹ This questioning eventually revealed, of course, that this "great dialogue" was a fiction; it had not taken place at all. Rather than a marvelous conversation carried on by successive generations of great thinkers, it was exposed as an intellectual construct that contemporary historians had devised in order to link the various books that they had come to think of as canonical. There were, as it turned out, virtually no true historical relations between the books themselves. As John Gunnell explained in 1979, "Over the years, by academic convention, a basic repertoire of works had been selected, arranged chronologically, represented as an actual historical [phenomenon], infused with evolutionary meaning, laden with significance derived from various symbolic themes and motifs, and offered up as the intellectual antecedents of contemporary . . . [patterns of] thought."⁸²

It was at this point that Skinner and Pocock, faced with questions that threatened to expose their discipline as a trick, tried to reconstruct intellectual history by providing its canonical works with what everyone could now see that they lacked: a genuine historical context. But the difficulties that their attempt has encountered, combined with the onslaught from poststructuralism, has left intellectual history with a canon but no historical explanation of its existence. Intellectual historians are left to rummage through a pile of bones that had once been the connected skeleton of their discipline. If intellectual history is to be reconstructed, this mound of bones will have to be given some form and shape, some overarching framework. We need something to take the place of our earlier belief that the great books were the products of an actual transgenerational dialogue of great thinkers. We need to give the canon a past, a historically conditioned identity.

We can find just what we need in the argument over biblical interpretation that first emerged during the Protestant Reformation. Central to the Reformation, of course, was Luther's conviction that every man was not only his own priest but his own interpreter. Behind this conviction lay two even more basic beliefs: first, that the scriptures were self-sufficient and self-interpreting, that they possessed a clear, fixed, and determinable meaning that rose directly out of

⁸¹ In "The Limits of Historical Explanation," *Philosophy*, 41 (1966): 199–215, for example, Skinner launched a devastating attack on "the influence model"—by which he meant the habit of treating earlier writers as if they were anticipating subsequent writers. Three years later, he offered a greatly expanded version of that critique in "Meaning and Understanding in the History of Ideas," *History and Theory*, 8 (1969): 3–53.

⁸² John Gunnell, *Political Theory: Tradition and Interpretation* (Lanham, Md., 1979), 68. It is surprising that this ideal construct should ever have been mistaken for a genuine historical tradition. In 1919, T. S. Eliot explained that "the existing monuments form an *ideal* order among themselves, which is modified by the introduction of the new (the really new) work among them"; Eliot, *Selected Essays* (London, 1934), 15, my emphasis. Nevertheless, both contemporary history of philosophy and contemporary philosophy of history have been very largely shaped (or misshaped) by the "revelation" Gunnell described. As David Hoy recently explained, "the really fundamental split in contemporary philosophy . . . is between those (like Dewey, Heidegger, Cavell, Kuhn, Habermas, and Foucault) who take Hegel and history seriously, and those who see 'recurring philosophical problems' being discussed by everybody"; Hoy, "Taking History Seriously: Foucault, Gadamer, Habermas," in *Union Seminary Quarterly Review*, 34 (Winter 1979): 85.

the text itself, without any assistance from the complicated interpretive apparatus that the church had gradually developed and now required; and, second, that the scriptures were meant to be taken literally rather than allegorically, as the church also insisted. "Origen's allegories are not worth so much dirt," as Luther put it; they are "the scum of Scripture."⁸³ For Reformation Protestantism, the church's insistence on interpretive procedures and allegorical meaning stood between Christians and their Bible, barring them from immediate access to the Word.

Skinner's and Pocock's proposal for reforming intellectual history is essentially a Protestant proposal. Like Luther, they argue that historical texts convey fixed meanings and that those meanings are accessible and ultimately determinable, if the critics or historians will only cut through the layers of interpretation that stand between the naked text and their inquiring minds. Like Luther, they look on these layers of accumulated interpretation as an impediment, an obstruction, an obstacle, barring historians from the "primary intentions" of the author. The wealth of interpretive material that surrounds a historical work they regard as so much scale-like incrustation that historians must smash to pieces with their "truly historical method" in order to get at the pearl of authentic meaning, what the author "really meant," what the text "really says."

The problems that this procedure have encountered suggest that what we need is an interpretive tradition erected not on the longing for authorial presence and invariable meaning, not on the illusion of the text as a "congealed intentionality waiting to be reexperienced," but on the recognition that every text, at the very moment of its inception, has already been cast onto the waters, that no text can ever hope to rejoin its father, that it is the fate of every text to take up the wanderings of a prodigal son that does not return. Interestingly enough, we can find such an approach in both the Rabbinic and the Catholic interpretive traditions.

Rabbinic Judaism begins not with the Word as the immaculate expression of God's will, the Word radiant with original meaning before every act of interpretation, but with Torah as the promise of *multiple* meaning, an invitation to continual interpretation and reinterpretation. It is not incarnation—the Word made flesh—but interpretation—the word made populous with meaning—that is the central divine act of Rabbinic Judaism. In every word there shines "an infinite multitude of lights."⁸⁴ As Emmanuel Levinas once explained, "it is precisely a *discourse*, not embodied in God, that assures us of a living God among us . . . The spiritual does not present itself as a tenable substance but, rather, through its absence; God is made real, not through incarnation but, rather, through the Law"—that is to say, through the text.⁸⁵ This is the concept Gershom Scholem called "pre-existent givenness," the idea that the Torah implicitly contains every interpretation that later commentators would eventu-

⁸³ Luther as quoted by Susan A. Handelman, *The Slayers of Moses: The Emergence of Rabbinic Interpretation in Modern Literary Theory* (Albany, N.Y., 1982), 123.

⁸⁴ Gershom Scholem, *The Messianic Idea in Judaism and Other Essays on Jewish Spirituality* (New York, 1971), 295.

⁸⁵ Emmanuel Levinas as quoted by Handelman, *Slayers of Moses*, 172, emphasis in the original.

ally discover. "So should it be that you would forsake Me, but would keep My Torah." As Rabbi Joshua ben Levi, a third-century Palestinian teacher, expressed it, "Torah, Mishnah, Talmud, and Aggadah—indeed even the comments some bright student will one day make to his teacher—were already given to Moses on Mount Sinai." And, as Scholem added, "even the *questions* that such a bright student will some day ask his teacher!"⁸⁶ Finally, there is the wonderful story of Rabbi Eliezer's quarrel with the Sages about the oven of Aknai. When God cried out, "Why do ye dispute with Rabbi Eliezer, seeing that in all matters the *halachah* agrees with him!" Rabbi Jeremiah replied, "The Torah has already been given at Mount Sinai; we pay no attention to a Heavenly Voice." At which point God "laughed with joy" and conceded "My sons have defeated Me, My sons have defeated Me."⁸⁷

In *The Slayers of Moses*, Susan Handelman contended that the entire history of Western criticism has been an argument between Jews and Christians over the hope of finding God in his Word, the author in his text. And that "there will always perhaps be war between Jews and Greeks, war over Scripture."⁸⁸ But, in the work of Brevard Childs, professor of Old Testament theology at Yale, we can find an interpretive strategy similar—though not identical—to the Rabbinic tradition she described. The work for which Childs is most widely known is *Introduction to the Old Testament as Scripture* (1979). This is Childs's response to Reformed orthodoxy's insistence on recovering the scriptures' aboriginal meaning. As Childs explained, "Modern canonical exegesis must be post-critical in nature. It does not seek to repristinate first century Christian interpretation" but "to understand the massive interpretative construal by means of which the sacred traditions" have been transmitted across so many generations.⁸⁹

By "massive interpretative construal," Childs means that, in the course of transmission from one generation to another, the sacred texts have been steadily reshaped, first by being gradually weaned from their original historical references, and second, by being drawn into larger textual complexes. In the course of this "traditioning process," the texts' primary meaning comes to be grounded in three ways: first, their reference to other texts in the larger textual complexes into which they have been drawn; second, their place within the larger interpretive apparatus that surrounds these textual complexes; and, finally, the new historical references they acquire from a combination of the first and second. It is through this "traditioning process" that sacred texts acquire their multi-dimensional character, their plenitude of meaning—the very qualities that identify them as canonical.

Gadamer made a similar point in his critique of traditional hermeneutics. Just

⁸⁶ As quoted by Scholem, *Messianic Idea*, 289. Frank Kermode made this notion of Torah the very definition of canonicity: "To be inside the canon is to be credited with indefinitely large numbers of possible internal relations and secrets, to be treated as a heterocosm, a miniature Torah"; Kermode, *Forms of Attention*, 90.

⁸⁷ Seder Nezikin, Tractate Baba Metzia 58b–59b (vol. 10, pp. 351–52 of Baba Metzia in the Soncino translation).

⁸⁸ Handelman, *Slayers of Moses*, 177.

⁸⁹ See Childs's critical review of James Barr, *Holy Scripture: Canon, Authority, Criticism* (Louisville, Ky., 1983) in *Interpretation*, 38 (1984): 69–70.

as the would-be interpreter's historically conditioned "prejudices" are not merely a series of obstacles to understanding but are, in fact, that which makes understanding possible, so the interpretations that have gradually accumulated around a particular text—or around the canon as a whole—are the only gateway we can possibly use to approach that text or collection of texts. According to Gadamer, "Understanding is never subjective behavior toward a given 'object,' but towards its effective history—the history of its influence."⁹⁰ In other words, interpretation forms the medium in which the text lives—the only medium in which it can live. Without the sustaining amniotic fluid of interpretation, the text would never have been born into our hands, would never have survived the reckless waste of time.⁹¹ Perhaps this is what T. S. Eliot was talking about in his famous 1919 article on tradition and the sense of history. Eliot argued that "the historical sense involves a perception, not only of the pastness of the past, but of its presence; the historical sense compels a man to write not merely with his own generation in his bones, but with a feeling that the whole of the literature of . . . his culture has a simultaneous existence and composes a simultaneous order."⁹²

From Barthes, LaCapra, and Kermode, we can construct a workable set of criteria for identifying the books with which intellectual history should concern itself; and, by drawing on the Rabbinic and Catholic interpretive traditions, and Gadamer and Eliot, we can give those works a historically grounded and historically conditioned identity. But what should intellectual historians do with these materials? What are our particular responsibilities?

IT HAS BEEN SAID THAT HISTORIANS bear a primary responsibility to the past, to the writers and thinkers we read and study, if for no other reason than because they lived through those times, and we did not.⁹³ Historians are said to carry out this obligation by listening to the people of the past, by trying to understand them in their own terms, and by telling us "what they really said."⁹⁴ There is no gainsaying the importance of making this effort. But from all that has gone before, it will be obvious that we should not expect actually to encounter now-dead authors in the body of their texts. The liberation of the text from "the glare of the father's eye," the process of cultural transmission, and the continual decontextualizing and recontextualizing that cultural transmission entails, all conspire to frustrate our hope that the text will be radiant with the presence of

⁹⁰ Gadamer, *Truth and Method*, xix.

⁹¹ Kermode, *Forms of Attention*, 36.

⁹² Eliot, *Selected Essays*, 14.

⁹³ E. P. Thompson, *The Making of the English Working Class* (New York, 1963), 12–13; Rorty, *Consequences of Pragmatism*, 200 and following, esp. 202.

⁹⁴ For a celebration of one contemporary historian's determination to do just that (in this case, to recover "the authentic, historical [William] James," to "confront James head-on, and to establish what his works said in the context in which he wrote them"), see Kloppenberg, "Deconstruction," 3–22, but esp. 18–19. Kloppenberg contrasted Hollinger's traditional commitment to the historical subject with LaCapra's "self-defeating" deconstructive approach. Kloppenberg feared that LaCapra's method would "make writing history impossible": 7.

the past.⁹⁵ Although we grant the text a privileged moral status, we cannot grant it a privileged epistemological status; the text—every text—will always be epistemologically inadequate.⁹⁶

Granted that historians bear a responsibility to those who lived in the past, our primary responsibility must be to those of us who live in the present. For, as Frank Kermode has explained, “we are necessarily more involved with the living than with the dead, with what learning cherishes and interpretation refreshes rather than with mere remains.”⁹⁷ It is for this reason that the historian must set out not merely to understand the writers of the past but to reeducate them, anachronistically imposing “enough of our problems and vocabulary on the dead to make them conversational partners.”⁹⁸ P. F. Strawson’s book *The Bounds of Sense* provides a good example. Strawson ripped Emmanuel Kant out of his historical context, stripped his thought of everything Strawson thought should not be there, and showed us that, for our purposes, living in our present, this refurbished Kantianism works better than the original.⁹⁹ As Richard Rorty commented, “Strawson’s conversation with Kant is the sort one has with somebody who is brilliantly and originally right about something dear to one’s heart, but who exasperatingly mixes up this topic with a lot of outdated foolishness.”¹⁰⁰

Historians do not generally condemn this sort of thing outright; they just do not think it is “history.”¹⁰¹ They regard the placing of historical actors in their “proper” contexts as “the first task of the historian,” the historian’s “first order of business,” “a matter of this propriety,” and so on.¹⁰² For intellectual historians, this means reconstructing the *mentalité* of a particular epoch, its central ideas and values, its modes of perception, its systems of discourse, its formal structures of thought, the ways in which it produced and disseminated meaning, and the procedures it used for translating meaning from one discourse to another. Most historians regard this sort of historical reconstruction as their primary responsibility. They believe that it is epistemologically impossible to understand the dead in *our* terms unless we first understand them in their own.¹⁰³

⁹⁵ Anna Smith, “The Death of the Critic?” *Untold*, 3 (September 1987): 46.

⁹⁶ The distinction between moral and epistemological privilege is Rorty’s, *Consequences of Pragmatism*, 202.

⁹⁷ Kermode, *Forms of Attention*, 75.

⁹⁸ Rorty in Rorty, Schneewind, and Skinner, *Philosophy in History*, 49.

⁹⁹ P. F. Strawson, *The Bounds of Sense: An Essay on Kant’s “Critique of Pure Reason”* (London, 1966).

¹⁰⁰ Rorty, *Philosophy in History*, 52. This is precisely the practice Skinner condemned as “The Mythology of Coherence.” See “Meaning and Understanding,” 16–20.

¹⁰¹ Pocock, for example, conceded that “restating the thought of ancients and predecessors in the language of one’s own day” may be a “proper” and “legitimate” activity but insisted that it is “obviously no part of the historian’s business”; *Politics, Language and Time*, 6, 8.

¹⁰² Toews, “Intellectual History after the Linguistic Turn,” 891; Haskell, “Reply by Thomas L. Haskell,” *Intellectual History Newsletter*, 3 (1981): 29; Skinner, “Meaning and Understanding,” 49. David Hollinger provides a good example of the position that has come to dominate intellectual history in America.

¹⁰³ Historians are not the only ones to think so. In *The Aims of Interpretation*, E. D. Hirsch distinguished “meaning” from “significance,” the former being “the author’s original meaning,” the meaning the author intended to convey, the latter referring to what later interpreters have found in the work (which Hirsch also called “anachronistic meaning”). Hirsch insisted that we cannot discuss a work’s “significance” until we have first determined its “meaning.” Hirsch, *The Aims of Interpretation*

I want to suggest an approach to the past that might serve as one alternative to this radical contextualism. I offer it not as a candidate for some new disciplinary paradigm that would ground and legitimize a universal methodology but merely as one approach among others. Moreover, there is nothing new in what I will suggest; I raise it only because it seems to have been misunderstood in recent discussions. Rather than have us groping our way backward toward some unreachable genesis, I want to suggest an approach that would abandon the attempt to recover authorial intention, that would be comparative rather than contextualist, that would concern itself not with the quest for textual origins but with the resituating of historical texts. In this view, intellectual history at its best would be "therapeutic rather than reconstructive, edifying rather than systematic."¹⁰⁴ Intellectual history as "quickenning power."¹⁰⁵ Here is Kermode's description of the *fin-de-siècle* historian Aby Warburg, whose scholarly research enshrined Botticelli among the pantheon of Renaissance painters: "Like most ambitious thinkers, [Warburg] used other men's thoughts and systems of ideas as stimulants rather than as schemes he might or might not adopt; he was not looking for something ready-made, but for hints, for the stimulus that might give rise to a brainwave of his own."¹⁰⁶ For Warburg, meaning unfolds in front of the text rather than behind it. Texts do not point backward, to the historical context or putative intentions of their now-dead authors; they point forward, to the hidden possibilities of the present.¹⁰⁷

Noam Chomsky's recovery and redeployment of Cartesian linguistics provides a good example. Until 1966, when Chomsky brought out *Cartesian Linguistics*, seventeenth-century philosophical grammar was "all but unknown."¹⁰⁸ Now, of course, some twenty years later, we know a great deal about that tradition, but it was Chomsky who pointed out its contemporary significance. What I want to emphasize here is the present-minded, even anti-contextualist nature of Chomsky's approach.¹⁰⁹ *Cartesian Linguistics*, as Chomsky described it, is "a projection

(Chicago, 1976), 79. Michael Ayers has drawn up a similar *modus operandi* for historians of philosophy. Ayers, "Analytical Philosophy and the History of Philosophy," in Jonathan Ree, Michael Ayers, and Adam Westoby, *Philosophy and Its Past* (Brighton, 1978). Quentin Skinner has gone even further, arguing that historians should avoid any discussion about the contemporary significance of the writers they study. Such discussion is "parasitic on the basic task of trying to recover how we think a given writer intended us to take his text"; Skinner, "Hermeneutics and the Role of History," 219.

¹⁰⁴ Rorty, *Philosophy and the Mirror of Nature* (Princeton, N.J., 1980), 5.

¹⁰⁵ The phrase is John Milton's, of course, by way of Harold Bloom, *The Breaking of the Vessels* (Chicago, 1982), 3.

¹⁰⁶ Kermode, *Forms of Attention*, 20.

¹⁰⁷ As Roland Barthes remarked, "A text's unity lies not in its origin but in its destination"; Barthes, *Pleasure of the Text*, 148. Paul Rabinow and William Sullivan were even more emphatic about this: "Let us be clear. What we want to understand is not something behind the cultural object, the text, but rather something in front of it"; Rabinow and Sullivan, eds., *Interpretive Social Science* (Berkeley, Calif., 1979), 12. For a discussion of legal interpretation along the same lines, see Ronald Dworkin, "Law as Interpretation," *Critical Inquiry*, 9 (September 1982): 193 and following. Susan Sontag once complained that the historian or commentator too often "digs *behind* the text, to find a subtext [or "discourse"] which is the true one . . . The modern style of interpretation excavates, and as it excavates, destroys"; Sontag, *Against Interpretation* (New York, 1966), 25.

¹⁰⁸ Noam Chomsky, *Language and Mind*, enl. edn. (New York, 1968, 1972), 16.

¹⁰⁹ For a stinging attack on Chomsky's "curious procedure in historical research and argument," see Hans Aarsleff, *From Locke to Saussure: Essays on the Study of Language and Intellectual History*

backwards of certain ideas of contemporary interest rather than a systematic presentation of the framework within which these ideas arose and found their place."¹¹⁰ As such, it makes "no attempt to characterize Cartesian linguistics as it saw itself but rather . . . concentrate[s] on the development of ideas that have reemerged, quite independently, in current work."¹¹¹ In other words, Chomsky turned to the past in order to find particular insights and discoveries that were "analogous" to contemporary developments—precisely the "weird tendency" that the contextualist historian John Dunn complained of so bitterly. Chomsky is quite clear about the difference between his own presentism and Dunn's contextualism: "I'm looking at history not as an antiquarian, who is interested in finding out and giving a precisely accurate account of what the thinking of the seventeenth century was—I don't mean to demean that activity, it's just not mine—but rather from the point of view of, let's say, an art lover, who wants to look at the seventeenth century to find in it things that are of particular value, and that obtain part of their value in part because of the perspective with which he approaches them."¹¹²

In *Cartesian Linguistics*, Chomsky returned to the early modern Port-Royal grammarians, evaluated their work in terms of its "contemporary significance," and showed us how it could be resituated, recontextualized, and put to new and important uses.¹¹³ He did something very similar two years later in *Language and Mind* which, in his own words, "illustrates how the [seventeenth-century] tradition of philosophical grammar can be reconstituted and turned to new and challenging problems."¹¹⁴ In each case, Chomsky set out to rescue an idea or an insight from the historical context into which it was about to disappear, resituated it in the context of contemporary research interests, and made us see that, thus resituated, it has the power to inform us about certain possibilities of the present—possibilities we had not seen before.

Michael Walzer's *Exodus and Revolution* provides another example of the sort of history I have in mind. *Exodus and Revolution* is a history of the Book of Exodus as a paradigm of revolutionary politics—indeed, as *the* engendering paradigm of virtually every revolutionary movement in the West, from the peasants' revolt in sixteenth-century Germany to the Puritan revolution in seventeenth-century England to the Boer revolt in southern Africa to the civil rights struggle in the American South to the liberation movements in contemporary Latin America. Walzer showed us that political thinkers of virtually every persuasion from Girolamo Savonarola to Benjamin Franklin to Karl Marx have

(Minneapolis, Minn., 1982), 101–19. Aarsleff took Chomsky to task for violating "the proper principles" of historical research, especially for failing to respect "the over-all coherence" of the period he studies (102). But, as will be apparent below, Aarsleff has criticized Chomsky for failing to write a book he had no intention of writing.

¹¹⁰ Noam Chomsky, *Cartesian Linguistics* (New York, 1966), 73.

¹¹¹ Chomsky, *Cartesian Linguistics*, 2.

¹¹² Chomsky in Elders, *Reflexive Water*, 143.

¹¹³ Chomsky, *Cartesian Linguistics*, 3. In the introduction, he wrote that he wants to discover new ways to "exploit" Cartesian linguistics.

¹¹⁴ Chomsky, *Language and Mind*, 23.

turned to the Exodus story. In other words, *Exodus and Revolution* is traditional intellectual history—an account of one particular idea as that idea has appeared in the thoughts of one thinker after another.

But it is more than that. As rabbinical commentators like to say of the Torah, “turn it and turn it again, for everything is in it.” One thing that is in *Exodus and Revolution* is an explicit attack on contextualist historiography. As one of the early reviewers noted, Walzer “is utterly uninterested in the findings of contemporary biblical scholarship, which undermine the stated meanings of the text in order to locate it in its ancient Israelite context.”¹¹⁵ Walzer described such efforts in his introduction, then dismissed them as outright failures.¹¹⁶ His own approach to the past is governed by quite a different set of assumptions: “In returning to the original text, I make no claims about the substantive intentions of its authors and editors, and I commit myself to no specific view of the actual history. What really happened? We don’t know. We have only this story, written down centuries after the events it describes. But the story [that is, the text] is more important than the events.”¹¹⁷

As this passage suggests, Walzer is less interested in the genealogy of the text than in the history of its meanings. But his project had little to do with the currently fashionable idea that intellectual history should explain the conditions governing the production and transmission of meaning; it was not an attempt to explain “why certain meanings arise, persist, and collapse at particular times and in specific sociocultural situations.”¹¹⁸ It was rather an effort “to grasp the meaning of the text through a critique of interpretations,” an attempt, in other words, “to discover its meaning in what it has meant.”¹¹⁹ *Exodus and Revolution* is a history of meaning rather than a history of the production and transmission of meaning.

But, if *Exodus and Revolution* is a history of what the Book of Exodus has meant to people living in the past, it is also an explanation of what it can mean to us, living in the present. For *Exodus and Revolution* shows us how the past illuminates and instructs the present, even as the present rewrites the past. When Walzer wrote that he was interested in “the radical potential of the Exodus story,” he meant he was interested in Exodus as a story that induces its readers to create their own stories, stories based on the original story but stories that are

¹¹⁵ *Virginia Quarterly Review*, 62 (Summer 1986): 531.

¹¹⁶ Michael Walzer, *Exodus and Revolution* (New York, 1985), 8.

¹¹⁷ Walzer, *Exodus and Revolution*, 7.

¹¹⁸ For a recent expression of the belief that intellectual historians must address themselves to “the investigation of the contextually situated production and transmission of meaning,” see Toews, “Intellectual History after the Linguistic Turn,” 882. Toews argued that intellectual historians “must address the issue of explanation, of why certain meanings arise, persist, and collapse at particular times and in specific sociocultural situations” (882), my emphasis. Compare this formulation with the question Walzer chose to ask: “Why is this story [Exodus] so endlessly reinvented? That is what I have tried to explain” (*Exodus and Revolution*, x). Toews’s question moves us outside the text, away from the story it tells; Walzer’s question can only be answered by moving *inside* the text, by asking, what is it about this particular text that has made it so generative for so many different peoples at so many different historical moments? Why is it that this particular story has made it possible for so many people to create so many stories of their own, each one germinated from the original text but each one new and uniquely empowering?

¹¹⁹ Walzer, *Exodus and Revolution*, 61, 7.

nevertheless “new and empowering,” stories that “engender human activity for a radical challenge to social justice.” Exodus has provided and continues to provide a narrative framework within which people can think about slavery and freedom, flight and deliverance, oppression and liberation. This may be, as Walzer admitted, a “deliberately anachronistic” approach to the past; but as he also remarked, “every reading [of the past] is also a construction, a reinvention of the past *for the sake of the present*.”¹²⁰

John P. Diggins’s book *The Bard of Savagery* offers a third and final example. Diggins is an intellectual historian with an interest in comparative social theory. He explained how Thorstein Veblen’s analysis of the historical development of capitalism, particularly his explanation of how unearned wealth is legitimated in capitalist society, differed from the analyses offered by Marx and Max Weber. He did this by purposefully lifting all three thinkers out of their particular historical contexts and strapping them onto a single intellectual grid, reading one thinker in terms of the other, asking them questions they had not thought to ask themselves. In this way, he was able to reveal certain limitations and exclusions, certain thoughts that remained unthought in each thinker’s work. Diggins developed this interrogatory, contestatory dialogue with the past—he called it “an exercise in theoretical confrontations”—not in order to criticize Marx and Weber for missing what Veblen grasped but rather because, like Chomsky, he wanted to rescue Veblen’s thought from the historical oblivion into which it seemed to be slipping; he wanted to return to it, rewrite it, resituate it, and make us see that, placed in a new and perhaps unexpected context, it has something interesting and valuable to show us—in this case, something about the historical development and contemporary staying power of modern capitalism that both Marx and Weber overlooked.

It is important to notice that, in doing so, Diggins purposely set out not only to recontextualize Veblen but to rewrite him. He filtered out certain aspects of Veblen’s work that seemed irrelevant to Diggins’s own concerns, and he underlined other elements in order to make them address those same concerns more sharply. Like Strawson rewriting Kant, Diggins wanted to help Veblen make himself presentable for a new audience. But it is also important to see that Diggins reeducated Veblen in order that Veblen might reeducate us, that he might show us what we had previously overlooked within our own present. Notice, too, that this was a game in which the stakes were particularly high: Diggins knew the risks he took by flying in the face of professional convention, but he also knew that if he could successfully recontextualize Veblen’s insights he might be able to effect a reevaluation not merely of Veblen’s own work but of contemporary social theory itself.¹²¹

But the American historical establishment would have none of it. Thomas Haskell, writing in *Reviews in American History*, rebuked Diggins for failing to “fix” Veblen “firmly in his times” and for the “arbitrariness” of his comparisons

¹²⁰ Walzer, *Exodus and Revolution*, 59, x, my emphasis.

¹²¹ John P. Diggins, *The Bard of Savagery: Thorstein Veblen and Modern Social Theory* (New York, 1978), xi.

with Marx and Weber. Diggins had placed Veblen “in an artificial context populated by a handful of major figures” who had been selected “on the basis of their importance to us, not Veblen.” Although “a contextualist account of Veblen might have been more pedestrian . . . it would have permitted a finer understanding of what Veblen actually said and meant.”¹²² Dorothy Ross, writing in the *AHR*, also complained that “the theorists examined and the questions asked of the material are generated not by history but by the author’s theoretical concerns.” Worst of all, Diggins seemed to feel that he was somehow “exempt” from the historian’s primary obligation: “to place Veblen in historical context.”¹²³

WHAT ARE WE TO MAKE OF ALL THIS? Two observations seem in order. First, context-oriented historians should stop chastising their colleagues for “presentism” and acknowledge the value—if not the necessity—of letting the present interrogate the past. To paraphrase Francis Bacon, we must put history to the rack; we must compel it to answer our questions.¹²⁴ *Our* questions, derived from our needs, couched in our terms.¹²⁵ Rorty once likened historians to anthropologists in a way that might be helpful here:

The anthropologist is not doing his job if he merely offers to teach us how to bicker with his favorite tribe, how to be initiated into their rituals, etc. What we want to be told is whether that tribe has anything interesting to tell us—interesting to *our* lights, answering to *our* concerns, informative about what *we* know to exist. Any anthropologist who rejected this assignment on the grounds that filtering and paraphrase would distort and betray the integrity of the tribe’s culture would no longer be an anthropologist, but a sort of cultist. He is, after all, working for *us*, not for *them*. Similarly, the historian of *X*, where *X* is something we know to be real and important, is working for those of us who share that knowledge, not for our unfortunate ancestors who did not.¹²⁶

Second, if recent developments in literary criticism and the philosophy of language have indeed undermined belief in a stable and determinable past, denied the possibility of recovering authorial intention, and challenged the plausibility of historical representation, then contextualist-minded historians should stop insisting that every historian’s “first order of business” must be to do

¹²² Haskell, “Veblen on Capitalism,” 559–60.

¹²³ Dorothy Ross, *AHR*, 84 (October 1979): 1179.

¹²⁴ Note, however, that this interrogation will necessarily be mediated by the defamiliarization of the present produced by our encounter with the past. See Gadamer’s discussion of the “fusion of horizons”; Gadamer, *Truth and Method*, 273 and following, 337, 358; and Anthony Giddens’s discussion of this “double hermeneutic” in Giddens, *New Rules of Sociological Method* (London, 1976), 158.

¹²⁵ By using the word “our,” I do not mean to imply a consensus concerning either questions or needs; I mean our *various and usually conflicting* questions, needs, and discourses. For an illuminating debate on this point, see Richard Bernstein, “One Step Forward, Two Steps Backward: Richard Rorty on Liberal Democracy and Philosophy,” a paper delivered at the Yale Law School Legal Theory Workshop, November 5, 1987 (mimeograph, pp. 22 and 24); and Rorty’s response, “Thugs and Theorists: A Reply to Bernstein,” mimeograph.

¹²⁶ Rorty, *Philosophy in History*, 6–7.

what now seems undoable.¹²⁷ Historians should simply drop the question of what counts as legitimate history and accept the fact that, like every other discipline in the humanities, they do not have, and are not likely to have, a formalized, widely accepted set of research procedures, and that nothing helpful or interesting is likely to come from attempts to define one.¹²⁸ If we ask, "what is historical writing?" the answer can only be, "there is this kind of historical writing, and that kind, and then again that kind." If such an understanding could win even grudging acceptance from the historical profession, a space might be cleared within which another sort of intellectual history could be written, a history concerned not with dead authors but with living books, not with a return of earlier writers to their historical contexts but with a reading of historical works in new and unexpected contexts, not with reconstructing the past but with providing the critical medium in which valuable works from the past might *survive* their past—might survive their past in order to tell us about our present. For only through such telling can we ever hope to see ourselves and our history anew.

¹²⁷ The conventional becomes "undoable," of course, only if one questions the epistemological assumptions on which the conventional has been built. But, as Kermode has pointed out, one need not ask such questions: "as long as we do things as they have generally been done—as long, that is, as the institution which guarantees our studies upholds the fictions which give them value—we shall continue to write historical narrative as if it were an altogether different matter from making fictions, or a *fortiori*, from telling lies"; Frank Kermode, *The Genesis of Secrecy: On the Interpretation of Narrative* (Cambridge, Mass., 1979), 109. One leading American historian who thinks historians ought *not* to ask such questions is Kloppenberg. He recently advised his colleagues to ignore the more virulent forms of contemporary criticism and get back to work. Like Roosevelt telling the nation that it has nothing to fear but fear itself, Kloppenberg wanted to reassure his colleagues that they can ride out the current wave of epistemological skepticism simply by maintaining "the courage of their conventions"; Kloppenberg, "Deconstruction," 8. Gordon Wood has also cautioned his colleagues against speculating about the "traditional" set of assumptions underlying and authorizing intellectual history. "Historians who cut loose from this faith," he warned, "do so at the peril of their discipline"; *New York Review of Books* (December 16, 1982): 59.

¹²⁸ Rorty, of course, has said much the same thing about philosophy. See *Consequences of Pragmatism*, 211–30, esp. 225–27. The most insistent call for such a paradigm for historical studies has come from Hollinger. He evidently believes that if an intellectual community expects to play an effective role in the larger culture it must organize itself around a set of shared commitments—commitments defined with enough rigor to "delimit problems, direct research, and establish criteria of judgment"; David Hollinger, *In the American Province: Studies in the History and Historiography of Ideas* (Bloomington, Ind., 1985), 177–81. Rabinow and Sullivan have characterized the enduring hope for such a paradigm as "the cargo-cult view of the 'about to arrive' science . . . The time seems ripe, even overdue, to announce that there is not going to be an age of paradigm in the social sciences"; Rabinow and Sullivan, *Interpretive Social Science*, 4. Derrida has also criticized the attempt "to reconstitute a system of essential predicates" that might govern the writing of history. "There is not one single history . . . but rather histories *different* as to their kind, their rhythm, their mode of inscription, unbalanced, differentiated histories, etc." Which is why he uses the word history "with quotation marks and precautions." Derrida, Interview with J.-L. Houdebine and Guy Scarpetta, *Diacritics*, 2 (Winter 1972): 42, 43. Nevertheless, Toews argued that we may, even now, be witnessing the emergence of just such a "consensual community"; Toews, "Intellectual History after the Linguistic Turn," 881 and following (but see also John Higham, *History: Professional Scholarship in America* [Baltimore, Md., 1983], 241).

AHR Forum
The Return of the Prodigal:
The Persistence of Historical Knowing

DAVID A. HOLLINGER

IF DAVID HARLAN IS CORRECT IN SAYING THAT “the most insistent call for . . . a paradigm” providing intellectual historians with “a formalized, widely accepted set of research procedures” has come from me,¹ Harlan’s fears about intolerance and “cargo-cult” scientism are without justification. The four pages he cites of my *In the American Province* make no such call. On the contrary, in these pages, I explicitly renounce the “reduction” of the craft of intellectual history “to form,” quote approvingly K. M. Minogue’s strictures against the degree of formalism found in the work of Quentin Skinner, disclaim any effort even to define the essential character of intellectual history, argue that this subdiscipline should be seen as a “commons” open to scholars of many orientations, and describe sympathetically—rather than prescribe—a series of commitments that “simply indicate the interests and sensitivities for which the study of American intellectual history has recently been a vehicle.”² I could be charged more plausibly with uncritical pluralism than with the formalization of a paradigm. But the meaning of a text, Harlan suggests elsewhere in his essay, can depend on the traditions of interpretation that develop around it. Is Harlan here giving us a demonstration of this process of meaning-production? My book is neither too recent nor too modest to qualify. After all, the process applies universally and it begins immediately: “every text, at the very moment of its inception, has already been cast onto the waters . . . no text can ever hope to rejoin its father . . . it is the fate of every text to take up the wanderings of a prodigal son that does not return.”³ As I understand the parable,⁴ the prodigal son’s wanderings were his own. Here, my “prodigal” text is being torn asunder and alienated from me. Does the “return of literature” license such abuse?

My experience with Harlan might be construed as sufficient cause to deal only mockingly with the notion that texts possess an agency, a power to do things

¹ David Harlan, “Intellectual History and the Return of Literature,” *AHR*, 94 (June 1989): n. 128; p. 609.

² David A. Hollinger, *In the American Province: Studies in the History and Historiography of Ideas* (Bloomington, Ind., 1985), 177–81.

³ Harlan, “Intellectual History,” 600.

⁴ Luke 15: 11–32. But perhaps I, with my ancestral Protestant interest in reading for myself, have not sufficiently applied myself to the biblical commentaries of which Harlan makes so much?

(such as traveling on the waters) previously attributed chiefly to people. But Harlan's inability to distinguish his own agency from that of the texts he cites need not prevent the rest of us from welcoming the return of literature. This shift in the winds of doctrine may help the historical profession to liberate itself more fully from an epistemological naïveté the development and perpetuation of which is the central theme of Peter Novick's extremely valuable new book, *That Noble Dream*.⁵ This shift may also increase our sensitivity to the influence of prior texts of many kinds on the written sources historians use. In order to profit from this literary self-awareness, however, we are obliged neither to share Harlan's poststructuralist enthusiasms nor to repudiate *wissenschaftliche* aspirations quite so thoroughly as Harlan encourages us to do. Nor need we accept Harlan's sense of what the subdiscipline of intellectual history can and cannot offer us.

Any scholar whose sources include traces left by the use of human language is presumably affected by the "extended epistemological crisis" that threatens "to reduce historical knowledge to a tissue of remnants and fabrications."⁶ Yet Harlan focuses exclusively on intellectual historians for reasons that remain obscure. Intellectual historians are more responsible than other historians "for keeping our cultural memory alive and our intellectual traditions relevant," he suggests.⁷ Whatever the merit of this observation, it fails to speak to the question of intellectual history's unique vulnerability to the epistemological crisis he attributes to "historical studies" in general.⁸ Are the claims a social historian might make about letters written between Polish peasants in Chicago and Cracow less subject to epistemological doubts than the claims an intellectual historian might make about the sermons of Samuel Willard? Are the judgments of political historians concerning the Constitutional Convention of 1787 more able to survive the postmodernist breakthrough than judgments intellectual historians might make about the Darwinian controversy? Surely, we are all in this together.⁹

It is true that scholars who call themselves "intellectual historians," or who are so labeled by others,¹⁰ are prominent among the historians now bringing about literature's "return" to the discipline.¹¹ These are the folks within the historian's

⁵ Peter Novick, *That Noble Dream: The "Objectivity Question" and the American Historical Profession* (New York, 1988). Much as I admire Novick's research and most of his analysis, I believe his final chapter—"There Was No King in Israel"—exaggerates the historical profession's fragmentation at the present time, and takes too apocalyptic a view of the destiny of historical knowledge.

⁶ Harlan, "Intellectual History," 581.

⁷ Harlan, "Intellectual History," 583.

⁸ Harlan, "Intellectual History," 581.

⁹ The relevance of contemporary literary theory to the entirety of historical studies, as normally practiced, is never lost from view in Robert F. Berkhofer, Jr., "The Challenge of Poetics to (Normal) Historical Practice," *Poetics Today*, 9 (1988): 435–52. For another example of a perspective broader than Harlan's, see an essay Harlan cites: Linda Orr, "The Revenge of Literature: A History of History," *New Literary History*, 18 (1986): 1–22.

¹⁰ J. G. A. Pocock, for example, is routinely counted a practitioner of "intellectual history" while professing to find the term unhelpful as a description of his own practice. See his "What Is Intellectual History," in Juliet Gardiner, ed., *What Is History Today* (London, 1988), 114.

¹¹ This historiographical fact is documented and assessed by John Toews, "Intellectual History after the Linguistic Turn: The Autonomy of Meaning and the Irreducibility of Experience," *AHR*, 92 (October 1987): 879–907.

guild who appear to spend the most time reading Michel Foucault and Jacques Derrida and Hans Georg Gadamer; these are the historians whose names most often appear in fliers for conferences in comparative literature and for cross-disciplinary symposia on such topics as "Whither the Humanities in the Age of Postmodernism?" This may well justify Harlan's focus on intellectual historians, but their leadership in the movement renders curious in the extreme Harlan's characterization of intellectual historians as the movement's chief victims. If literary-theoretical consciousness is to become more highly valued in historical scholarship, and if such consciousness currently resides more in intellectual history than in other subdisciplines, the opportunities intellectual historians will have to contribute to the discipline of history are likely to increase. Dominick LaCapra has controversially sought to defend and redirect the craft of intellectual history by drawing on some of the ideas that excite Harlan, but LaCapra enters Harlan's story as a critic rather than an advocate of "intellectual history." This term usually turns out for Harlan to mean what he sometimes calls "contextualism," a persuasion he identifies especially with Quentin Skinner and J. G. A. Pocock. Intellectual historians, like other clusters of scholars, do disagree among themselves. The differences among intellectual historians over specific theories concerning the power and contingency of language are real,¹² but Harlan misleads when he depicts these disagreements as attacks on intellectual history as such.¹³ Indeed, much of Harlan's essay is simply an episode in this continuing discussion among intellectual historians: it is a polemical attack on "contextualism" in the guise of a general reconsideration of the "return of literature."

It may be helpful to remember that the persuasion we have come to call "contextualism" was not originally articulated in response to Gadamer, whose splendid historical sensitivity Harlan uses unfairly against Skinner. Contextualism arose in reaction to complacently Whiggish and ahistorical perspectives decidedly more distant than Skinner is from the insights developed by Gadamer. The most influential methodological writings of contextualism were generated in the 1960s, when basic historicist ideas were transferred from their traditional scholarly space into studies of political thought, philosophy, social science, and natural science. In some of these quarters, it was an advance to apply the simple Collingwoodian postulate that understanding an argument depends in part on discovering what questions the argument was intended to answer. Among the many virtues of Thomas S. Kuhn's *Structure of Scientific Revolutions* (1962) was its demonstration that the rigorous application of historicist ideas to the natural sciences could greatly enrich our understanding of scientific development. Although the work of Pocock and Skinner in the history of political thought and

¹² For a vigorous exchange, see James T. Kloppenberg, "Deconstruction and Hermeneutic Strategies for Intellectual History," *Intellectual History Newsletter*, 9 (1987): 2–22; and Dominick LaCapra, "Of Lumpers and Readers," *ibid.*, 10 (1988): 3–10. A judicious overview of current controversies concerning intellectual history is Donald R. Kelley, "Horizons of Intellectual History: Retrospect, Circumspect, Prospect," *Journal of the History of Ideas*, 48 (1987): 143–69. Toews, "Linguistic Turn," is a remarkably wide-ranging and penetrating discussion of the issues.

¹³ Harlan, "Intellectual History," *passim* but esp. 597.

the work of George Stocking in the history of social science drew on other sources of insight as well, Kuhn's historicism was an acknowledged inspiration for the widely quoted contextualist declarations of all three of these scholars during the late 1960s and very early 1970s. Most of these early pieces, like Kuhn's great book, contained elements of hyperbole later regretted by their authors and endlessly quoted by their critics. Yet some of these early formulations have continued to be drawn on and critically revised during the 1970s and 1980s by a number of American historians, including myself, seeking to vindicate the tradition of intellectual history best exemplified by Perry Miller's work of the 1930s, 1940s, and 1950s. In the meantime, Skinner has become entangled in an increasingly technical debate with political philosophers over the nature and significance of authorial intention and over the relevance to historical study of the "speech act" theories developed by J. L. Austin and John Searle.¹⁴

The centrality of Kuhn to this story is worth emphasizing, because Harlan, even after acknowledging Skinner's responsiveness to the new philosophy of science,¹⁵ considers "devastating" to contextualism a point of Gadamer's that can be plausibly construed as a post-Kuhnian cliché: "the historians' preconceptions and prejudices are what make understanding possible in the first place," and cannot be simply overthrown in the interest of getting inside the mind of a historical figure.¹⁶ No one has done more than Kuhn to make us aware of the power, the historicity, the contingency, and the indispensability of "preconceptions." The extent to which one can reasonably hope to reconstruct Isaac Newton or Thomas Hobbes or Aristotle each on his "own terms" is a matter of disagreement, but historians of the contextualist persuasion have generally been content to labor within Wilhelm Dilthey's "hermeneutic circle": interpretations are "circular" in that they cannot be expected to touch an Archimedian point. The "progress" made is thus not of the positivist sort. Gadamer believes that Dilthey and his predecessor, Friedrich Schleiermacher, have failed to see how tightly constraining this circle is.¹⁷ It is Gadamer's critique of this Schleiermacher-Dilthey tradition that Harlan now uses against Skinner and his fellow contextualists, accusing the whole lot of being, in effect, crude positivists: how naïve of them to think they can escape the hermeneutic circle, discover true historical objects, and thereby reconstruct with scientific exactitude the mind of Abraham Lincoln or John Locke! Contextualists may have taken their Kuhnian heritage too much for granted and may have spoken incautiously about

¹⁴ A recent volume collects several of the most noted of the critiques of Skinner, and includes his lengthy reply: James Tully, ed., *Meaning and Context: Quentin Skinner and His Critics* (Princeton, N.J., 1989). This book appeared after Harlan prepared his essay for the *AHR*. Skinner's responses to several of his other critics are relevant to some of Harlan's complaints, however, and render less necessary a defense of Skinner on my part. Especially in answering John Keane does Skinner take up issues of concern to Harlan; see Skinner, "A Reply to My Critics," in Tully, *Meaning and Context*, 268–69, 272–73, 279–81.

¹⁵ Harlan, "Intellectual History," 583–84.

¹⁶ Harlan, "Intellectual History," 587.

¹⁷ In appreciating Gadamer's role in the history of German hermeneutics, I have been helped by the lucid introduction David E. Linge has done for a volume of Gadamer's essays: Hans Georg Gadamer, *Philosophical Hermeneutics*, David E. Linge, trans. and ed. (Berkeley, Calif., 1976), xi–lviii, esp. xii–xiv.

reconstructing the past. But a little caution in these prefaces and methodological essays (a judiciously positioned quotation from William James about the “tail of the human serpent” covering all?) would appear to be all that is required to neutralize Harlan’s Gadamer-based critique. Unless, of course, one believes that contextualist scholarship itself really is oblivious to the modern epistemological currents contextualism’s own leaders are often credited with spreading within the historical profession. And what *about* contextualist scholarship? How positivistic is it? Can an epistemologically sophisticated reader learn anything from this scholarship?

These questions bring me to the most remarkable feature of Harlan’s attack: his apparent belief that purely philosophical arguments are all that is required to dismiss as an impossibility the entire project of studying ideas in their historical context. We are to believe that this project is so wrong-headed in theory, and so inadequately defended, that there is no need to examine its monographic results even for the purpose of clarifying the meaning of its methodological commentaries. Harlan makes no critique of Pocock’s actual studies of the history of republicanism but finds Pocock’s endeavor at recovering “men and women thinking” so crashingly naïve as to call forth the image of Ahab trying to pierce the “empty whiteness,” the “maddening blankness” of the white whale.¹⁸ Harlan does not fault Skinner’s *Foundations of Modern Political Thought* but dismisses Skinner on the prior grounds that any work fairly described by Skinner’s methodological writings could not possibly have any chance of success. Harlan states no objection to my interpretation of William James but finds it quaint that I should even try to establish what James said in the context in which he spoke. Throughout, Harlan treats the theoretical discussion of intellectual history as if it were an autonomous discourse, rather than a sustained commentary on practice.

HARLAN’S ALOOFNESS FROM MONOGRAPHIC HISTORICAL STUDIES renders exceedingly abstract his dismissal of the “new discourse-oriented historians who now dominate the writing of American intellectual history.” These plodders seem not to have worried enough about the diminution of authorial presence and volition entailed by rigorous attention to context. Deaf to poststructuralist reports of the “death of the author,” they “simply assume, without much discussion, that they can submerge the subject in a history of discourses yet somehow retain the subject as an intending agent.”¹⁹ Is it too much to ask that one look at the efforts these historians have made to work out the role of specific intending subjects in specific contexts of discussion? What about Richard Fox’s *Reinhold Niebuhr: A Biography* (1985), Daniel J. Singal’s *War Within: From Victorian to Modernist Thought in the South, 1919–1945* (1982), James T. Kloppenberg’s *Uncertain Victory: Social Democracy and Progressivism in European and American Thought*,

¹⁸ Harlan, “Intellectual History,” 592.

¹⁹ Harlan, “Intellectual History,” 593.

1870–1920 (1986), Louise L. Stevenson's *Scholarly Means to Evangelical Ends: The New Haven Scholars and the Transformation of Higher Learning in America, 1830–1890* (1986), Bruce Kuklick's *Rise of American Philosophy: Cambridge, Massachusetts, 1860–1930* (1977), Howard Brick's *Daniel Bell and the Decline of Intellectual Radicalism: Social Theory and Political Reconciliation in the 1940s* (1986), or my own *Morris R. Cohen and the Scientific Ideal* (1975)? If these are not “contextualist” works in American intellectual history, Harlan's use of this category is very refined indeed. Are any or all of these books unpersuasive in the degree of authorial presence and volition attributed to the various individuals that make up each book's cast of characters? Perhaps they are, but Harlan gives us no reason to suppose that these works, or any other particular works, are deficient in this respect.²⁰ Harlan's evidence is merely the failure of more historians to display Thomas Haskell's concern about the “deterministic implications” of current conventions. Voluntarism and determinism, yes, a tough old question. Must historical research and writing be put on hold until this philosophical problem is solved? Or are we obliged to believe that Roland Barthes has already solved it?

Many of the great issues in modern thought are indeed solved, Harlan implies. Is there no serious opposition to the most sweeping of the claims of the poststructuralists? On the basis of Harlan's essay, one would never suspect that these claims have failed to win the support of any segment of the cognoscenti other than the historians Harlan attacks. Hence the persistent invidiousness in Harlan's treatment of critical exchanges: any grunt from a context-oriented scholar to the effect that another historian has failed to attend sufficiently to context is an example of “intolerance,” while no such narrowness of mind is carried by charges that contextualist scholarship is a “trick,” a “mystery religion without a gospel,” and simply “undoable.” Was David Hume “intolerant” when he proposed the casting into the flames of books containing no “abstract reasoning concerning quantity or number . . . [or] experimental reasoning concerning matters of fact and existence”? No, Hume was simply criticizing the neanderthals of his day on the basis of sound, up-to-date principles.

I am unmoved by Harlan's accusation that contextualists are “increasingly intolerant” and by his implication that they as a group are acting in the profession to stifle “alternative approaches.”²¹ Reviews done ten years ago of John P. Diggins's book on Veblen will scarcely bear the weight Harlan assigns to them; and if the authors of those reviews—Haskell and Dorothy Ross—are part of the “establishment,” so, too, is Diggins. Is it too idealistic to suggest that contextualism has thrived not as a result of the intolerance of its practitioners but because of its appeal to many scholars as a credible and fruitful way to do intellectual history? Be that as it may, Harlan's conclusion provides fascinating

²⁰ Dominick LaCapra's *History and Criticism* (Ithaca, N.Y., 1985) contrasts strikingly with Harlan's aloofness from monographic work. In some chapters of that book, LaCapra tried to show that certain theoretical assumptions operate with bad effects within specific texts written by Carlo Ginzburg, Carl Schorske, and Robert Darnton. LaCapra does not persuade me in every case, but I find the level of his engagement appropriate and challenging.

²¹ Harlan, “Intellectual History,” 594.

evidence that contextualism is less narrow, less embattled, and more responsive to recent theoretical innovation than he allows. When summing up his critique and pointing the way to something new, Harlan turns predictably to the self-styled “postmodernist” Richard Rorty, the pluralistic, eclectic American sponsor of a great variety of contemporary theorists, including Derrida and others who have inspired Harlan. Harlan attributes to Rorty a long quotation likening historians to anthropologists and reminding everyone that historical study is carried out for our own benefit, not the benefit of historical actors. How fitting a contrast to those closed-minded contextualists. Yet the passage could just as fairly have been attributed to the arch-contextualist Skinner: it is from the preface to *Philosophy in History*, signed by its three editors, Rorty, Skinner, and J. B. Schneewind.²²

If Harlan’s sense of the intellectual and professional relations of contextualists to other scholars demands correction, so, too, does his sense of what is at issue in the relevant methodological discussions of intellectual history. He seems to believe the chief aim of intellectual historians has been, and must be, the justification of canons and the critical commentary on the specific texts regarded as canonical. Harlan betrays this assumption at a number of points. If “the literary claims lodged against intellectual history are true,” Harlan explains, “its canon” would be “exposed as a pretentious imposition on the past.” By acting on the insights of “Barthes, LaCapra, and Kermode,” he writes of the future, “we can patch together provisional, working criteria for identifying the books with which intellectual history might concern itself.” He takes the salient question to be, “with which texts should intellectual historians concern themselves?”²³ But, in fact, this question generally follows prior questions, which serve to define the particular project a scholar may be carrying out. If you want to know how the second and third generation of New England Congregationalist leaders coped with challenges to the idea of the covenant, you are likely to focus on certain texts (those written by New England Congregationalist preachers between 1660 and 1730) rather than others as the chief sources for your study. Your choice of that inquiry to begin with may or may not derive from a suspicion that the relevant sources will be “writerly” or “readerly” in Barthes’s sense, “complex” or “documentary” in LaCapra’s sense, or “canonical” in Kermode’s sense, but the example of Perry Miller’s *New England Mind*²⁴ suggests that much of what intellectual historians do would fit awkwardly into Harlan’s canon-defined framework. I doubt if that framework could easily accommodate three well-known, diverse, recent projects generally recognized as “intellectual history”: Keith Baker’s “Ideological Origins of the French Revolution,” James R. Moore’s *Post-Darwinian Controversies*, and Christa

²² Richard Rorty, J. B. Schneewind, and Quentin Skinner, eds., *Philosophy in History* (Cambridge, 1984). This book served to launch the most visible and energetic reassertion of contextualist principles in recent years, the “Ideas in Context” series, a signal contribution to which is Novick’s *That Noble Dream*. The contextualism displayed by the sixteen contributors to *Philosophy and History* is considerably less restrictive, moreover, than are the passages from Skinner’s 1975 essay, “Hermeneutics and the Role of History,” of which Harlan makes so much. The “Ideas in Context” series is turning out to be commodious indeed.

²³ Harlan, “Intellectual History,” 597, 598, 597.

²⁴ Perry Miller, *The New England Mind: From Colony to Province* (Cambridge, Mass., 1953).

Jungnickel and Russell McCormach's *Intellectual Mastery of Nature*.²⁵ Many works in intellectual history are addressed to a "middle ground" of discourse, positioned in our conventional cultural hierarchy "below" the classical works we associated with canons in literature and philosophy, yet "above" the culture of everyday life.²⁶ Canons are more likely to interest intellectual historians as problems for analysis: how and why did a given collection of texts come to be regarded as a "canon"?²⁷

THE TENDENCY OF INTELLECTUAL HISTORIANS to treat canons not as objects of worship but as problems for analysis, and the attention many of these historians devote to "middle" rather than "high" discourse, lends some credibility to the complaint made by Harlan that "complex works" are reduced "to the status of documents."²⁸ I sought to deal with this issue ten years ago in an essay Harlan now misrepresents as an effort on my part to "go even further" in a direction since criticized by LaCapra, the downgrading of "the importance of reading and interpreting complex texts." "Hollinger . . . would have us approach complex texts in a thoroughly instrumental fashion, using them to illuminate the discourse of which they are but manifestations," according to Harlan.²⁹ In fact, I tried to protect the special characteristics of complex texts from such reduction by distinguishing between two kinds of contextual study, one designed to enrich readings of particular texts, especially but not exclusively great ones such as Hobbes's *Leviathan*, and the other designed to illuminate a larger discourse in which such a text may have been a part. It is one thing, I insisted, to study discourse "as a means of interpreting particular texts" and another to study that larger discourse itself. An utterance of T. H. Huxley or Charles Darwin can be a "central subject," but, so, too, can the Darwinian controversy. I cautioned against the idea that "either of these two enterprises can go forward without the insights derived from the other," hoping for a comfortable and mutually

²⁵ Keith Baker, "The Ideological Origins of the French Revolution," in Dominick LaCapra and Steven L. Kaplan, eds., *Modern European Intellectual History: Reappraisals and New Perspectives* (Ithaca, N.Y., 1982), 197–219, esp. 197–200, where Baker cogently sets forth his perspective on "intellectual history"; James R. Moore, *The Post-Darwinian Controversies: A Study of the Protestant Struggle to come to grips with Darwin in Great Britain and America, 1870–1900* (Cambridge, 1979); Christa Jungnickel and Russell McCormach, *Intellectual Mastery of Nature: Theoretical Physics from Ohm to Einstein*, 2 vols. (Chicago, 1986).

²⁶ This "middle ground" has been ably analyzed in David F. Lindenfeld, "On Systems and Embodiments as Categories for Intellectual History," *History and Theory*, 27 (1988): 30–50, esp. 34–35, 49. Although I do not share the concern Lindenfeld and many others have voiced that intellectual history needs to assert its "autonomy" by developing a more distinctive *problématique*, I find this article helpful as a clarification of what does, in fact, distinguish many of the projects we call "intellectual history" from other types of historical study. The value of Lindenfeld's discussion seems all the greater in view of the persistence, exemplified by Harlan, of the misconception that canons are as important to intellectual history as they are to literary criticism, philosophy, and theology. For an important consideration of conventional cultural hierarchies, see Lawrence W. Levine, *High Brow/Low Brow: The Emergence of Cultural Hierarchy in America* (Cambridge, Mass., 1988).

²⁷ Two recent examples are Bruce Kuklick, "Seven Thinkers and How They Grew: Descartes, Spinoza, Leibniz; Locke, Berkeley, Hume; Kant," in Rorty, Schneewind, and Skinner, *Philosophy in History*, 125–39; and David A. Hollinger, "The Canon and Its Keepers: Modernism and Mid-Twentieth-Century American Intellectuals," in Hollinger, *Province*, 74–91.

²⁸ Harlan, "Intellectual History," 596.

²⁹ Harlan, "Intellectual History," n. 69.

supportive division of labor. Still fearful that some future David Harlan might miss the point, I made it more emphatic: if historians of political philosophy “did not focus on texts like Hobbes’s,” I preached, “we would certainly need to create a discipline that did.”³⁰ If only I had read more literary theory back then, I would have known better than to try so hard to keep that essay, too, from becoming a prodigal.³¹

However dubious Harlan’s constructions of recent historiography, his vision of what lies ahead of us—as we navigate in the wake of literature’s return to our waters—is anticlimactic. We do not need the poststructuralists to enable us to appreciate the virtues of Noam Chomsky, Michael Walzer, and John P. Diggins. Nor do we need, toward this end, the services of the priests and rabbis whose venerable hermeneutic practices Harlan portentously calls to our attention. In modern academia, we have always been surrounded by a variety of kinds of critically engaged studies of ideas, movements, texts, and individual intellectuals from the past. When these studies are intelligent and learned, as they often are, the benefits are real for nonhistorians and historians alike. Contextualism tried to make an additional contribution that trained historians were presumably the best qualified to offer: increased access to history itself, to the meaning of ideas in their historical context. No one ever claimed, to my knowledge, that our access to texts should be exclusively that yielded by this kind of rigorous, contextual study. The sorts of interpretation Harlan now labors so hard to license have always been ubiquitous. Contextualism went beyond them but without closing them off. The project of studying ideas in their historical context tried to liberate us, to whatever degree proved possible, from the constraints of the present. One major value of contextualism was the expansion of our perspectives on texts, in order that we get more, not less, from these texts. “The anachronistic readings” of William James that result from the projection onto James of subsequent

³⁰ David A. Hollinger, “Historians and the Discourse of Intellectuals,” in John Higham and Paul K. Conkin, eds., *New Directions in American Intellectual History* (Baltimore, Md., 1979), 44, 52.

³¹ Readers tire of authors’ protests about being misrepresented, so I will confine to this footnote other complaints I want to make for the record about Harlan’s use of my “Historians and the Discourse of Intellectuals.” The “most obvious and pressing” problem faced by contextualism, Harlan explains, is “defining the relevant ‘network of intellectual discourse’ with any precision”; Harlan, “Intellectual History,” 594. I am quoted as someone who has failed to confront this problem effectively: “Hollinger . . . conceded that the relevant context may have to be defined” so broadly as to include just about everything to which the educated people of a given society had access at a given time. Yet my reference to this cognitive panorama spoke not to the setting of boundaries of a given discourse under study; it spoke rather to the issue of what distinguishes “intellectuals” from other people whose discourse historians study (see “Discourse of Intellectuals,” 55). Elsewhere in the essay, I do confront, at some length, the issue that concerns Harlan. I emphasize “shared questions” as a basis for identifying particular communities of discourse (“Discourse of Intellectuals,” 43), and I seek, through commentaries on a number of specific monographs (“Discourse of Intellectuals,” 45–54), to illustrate what might count as “shared questions.” No hint of this gets into Harlan’s account. In yet another place (594), Harlan cites this essay of mine as insisting that any given text can “only” be understood by placing it in a specific context of public discussion. My praise of John Kasson’s sensitivity to context (“Discourse of Intellectuals,” 52, miscited by Harlan as 54) cannot be fairly translated into any such sweeping claim. Harlan continues with the observation of LaCapra that all this talk about “networks of intellectual discourse” means “in practice” studies of “the various specialized discourses of intellectuals” (594). LaCapra is of course right, and I am, too. Harlan seems to have missed my starting point: the explicit aim of my article on the “Discourse of Intellectuals” was to comment on the particular kind of intellectual history addressed to (guess what?) the discourse of intellectuals.

discourse “are not difficult to live with,” I once tried to explain, “but a more accurate comprehension of what James was doing can make a more authentic James available for use in these contemporary debates.”³² It is a mark of contextualism’s credibility that scholars wanting to eschew historical readings began in the 1960s to make more frequently the distinction between historical and nonhistorical studies. Chomsky’s disclaimer about doing history, noted by Harlan, is a convenient example. The more recent, self-conscious efforts to produce “creative anachronisms” are also evidence of heightened awareness of the distinction. To affirm the virtues of contextual study need not entail a lack of respect for the Walzers of our world, although it may well produce some illuminating, critical exchanges on what we learn or do not learn from studies like his. Harlan, not otherwise distinguished for his skill with understatement, does well when he says of his Chomsky-Walzer-Diggins section, “there is nothing new in what I will suggest.”³³

Yet, in the wake of poststructuralism, there are some new things happening to which Harlan seems oddly oblivious. His essay would have been more challenging had he treated literature’s return not as a license for the old but as a charter for the new. I am less qualified than he presumably is to make this case, so I will simply allude to some recent works that, while very different from one another, do appear to have brought into the actual doing of intellectual history some of the insights into human language that excite Harlan. The extraordinary essays of Walter Benn Michaels, collected in *The Gold Standard and the Logic of Naturalism*, provide some terrific grist for the immersion-of-the-intending-subject-in-discourse mill. “The subject of naturalism,” Michaels argued in a sentence that gains power in the context of the several pages preceding it, “is typically unable to keep his beliefs lined up with his interests for more than two or three pages at a time,” on account of “the fact that his identity as a subject consists only in the beliefs and desires made available by the naturalist logic—which is not produced by the naturalist subject but rather is the condition of his existence.”³⁴ For other indications of the promise held for intellectual history by the “return of literature,” one might turn to Jean-Christophe Agnew’s *Worlds Apart*, John Owen King’s *Iron of Melancholy*, and Allan Megill’s *Prophets of Extremity*.³⁵

³² Hollinger, *Province*, 3. In my effort to explain the purpose of a contextualist study of James, I added another reason, more peculiar to historians: the goal of “getting right” the intellectual history of the United States. This is the kind of language that puts me in trouble with Harlan. I take consolation in the fact that other scholars, more sophisticated philosophically than I, sometimes use the same phrase with conviction. See, for example, two provocative essays in a leading literary journal’s informative symposium on “Philosophy of Science and Literary Theory”: Hilary Putnam, “A Comparison of Something with Something Else,” *New Literary History*, 17 (1985): 79; Martha Nussbaum, “Sophistry about Conventions,” *ibid.*, 129.

³³ Harlan, “Intellectual History,” 604.

³⁴ Walter Benn Michaels, *The Gold Standard and the Logic of Naturalism: American Literature at the Turn of the Century* (Berkeley, Calif., 1987), 177. For an intelligent critique of Michaels, see Evan Carton, “American Literary Histories As a Social Practice,” *Raritan*, 8 (1989): 127–32.

³⁵ Jean-Christophe Agnew, *Worlds Apart: The Market and the Theater in Anglo-American Thought, 1550–1750* (Cambridge, 1986); John Owen King III, *The Iron of Melancholy: Structures of Spiritual Conversion in America from the Puritan Conscience to Victorian Neurosis* (Middletown, Conn., 1983); Allan Megill, *Prophets of Extremity: Nietzsche, Heidegger, Foucault, Derrida* (Berkeley, Calif., 1985).

The positive result of the return of literature most exciting to Harlan, it seems, is simply the opportunity to legitimize a variety of kinds of scholarship. "There is this kind of historical writing, and that kind, and then again that kind," he exults.³⁶ Harlan pays little attention to the basis, or bases, on which one might evaluate the relative success of any particular work in any of the genres of history Harlan wishes to embrace. How do we know that what Walzer says is true? If this is not a question we should ask, what makes the question mistaken? Some of the theorists to whose work Harlan directs our attention have guidance to offer here, but Harlan makes little use of it. Is the "epistemological crisis" of which he speaks so deep as to render premature any discussion of critical evaluation? Hans Reichenbach's *Rise of Scientific Philosophy* is a famous, contentious example of the kind of history Harlan appears to be defending.³⁷ On what grounds might one critically evaluate Reichenbach's storytelling? Is it a fine story for positivists to believe in but not for the rest of us, who are simply free to write our own stories? Does it matter that stories conflict? What about racist stories? What about studies that apparently refute racist stories: Martin Bernal's *Black Athena*?³⁸ Does the return of literature affect the ways in which we evaluate Bernal's claims, evidence, and reasoning concerning the origins of Greek culture and concerning the constructions of those origins offered by modern scholars?³⁹

Trends in literary theory do come and go with some rapidity. When in the presence of a colleague in comparative literature recently, I made what I took to be an unproblematic reference to poststructuralism. My colleague was amused to find that I was so far behind the times. "Oh, but the thing now is post-poststructuralism," she said with evident sincerity. "What's that?" I asked. "Putting history back in, but not just facts, the way historians would do it," she explained with a touch of impatience. I pressed for details, but she changed the subject. As these fashions come and go, historians are justified in reacting with caution. By reserving judgment while exploring the resources of modern literary theory, historians need not feel guilty of anti-theoretical Philistinism. "Reserving judgments is a matter of infinite hope," Nick Carraway once pointed out.⁴⁰ The hope in our case is twofold. We historians can hope, first, to learn from these theories new ways in which to improve the history we write and to better explain it to our contemporaries. We can hope, second, to preserve in the old historian's craft a cognitive element, which some of these theories invite us to give up as a lost prodigal.⁴¹

³⁶ Harlan, "Intellectual History," 609.

³⁷ Hans Reichenbach, *The Rise of Scientific Philosophy* (Berkeley, Calif., 1951).

³⁸ Martin Bernal, *Black Athena: The Afroasiatic Roots of Classical Civilization*, Vol. 1, *The Fabrication of Ancient Greece, 1785–1985* (New Brunswick, N.J., 1987).

³⁹ For some anxious ruminations on "postmodern criteria for what constitutes historical knowledge," stimulated in part by revisionist accounts of Auschwitz, see Charles S. Maier, *The Unmasterable Past: History, Holocaust, and German National Identity* (Cambridge, Mass., 1988), esp. 170.

⁴⁰ F. Scott Fitzgerald, *The Great Gatsby* (New York, 1925), 1.

⁴¹ Both of these hopes are reinforced by a recent, highly relevant discussion of narrativity and knowledge not mentioned by Harlan: Richard Harvey Brown, "Positivism, Relativism, and Narrative in the Logic of the Historical Sciences," *AHR*, 92 (October 1987): 908–20.

PERHAPS SOMEONE WILL DEVELOP A THEORETICAL ACCOUNT of this “cognitive element” able to command the respect of the men and women currently in the thrall of philosophies that “reduce historical knowledge to a tissue of remnants and fabrications concealing,” as Harlan summarizes the challenge, “an essential absence.”⁴² I have not attempted any such account here, but have instead turned repeatedly to examples of existing historical scholarship, asking that Harlan’s concerns be translated into the terms of these concrete products of research and writing. If this procedure inevitably begs some philosophical questions, I believe it also forces these questions onto a terrain on which historians are more likely to engage them actively even as they continue to produce history. “After the philosophical smoke clears,” the philosopher Cornel West has reminded us, “the crucial task” is to actually do it, to deliver “historical accounts for the emergence, development, sustenance, and decline of vocabularies and practices in the natural and human sciences against the background of dynamic changes in specific (and often coexisting) modes of production, political conflicts, cultural configurations, and personal turmoil.”⁴³ What historians can contribute is not limited to projects of the sort West describes in this call to monographic action, but the call carries a healthy sense of urgency I find altogether missing in Harlan’s essay.

I do not know what ultimately explains our vocabularies and our practices, but I believe we must put at risk in research the theories we find most promising. I do not possess a calculus for measuring the relative agency of authorial intention, but I believe that authors remain present enough in texts to justify our listening for their voices. I do not understand the mystery of knowing, but I believe this mystery has survived the return of literature.

⁴² Harlan, “Intellectual History,” 581. Such philosophies have become so strong, it has been suggested, that by the 1980s “hardly anybody was listening” to efforts at a “middle-of-the-road grounding of the historical venture”; Novick, *That Noble Dream*, 628.

⁴³ Cornel West, “Afterward,” in John Rajchman and Cornel West, eds., *Post-Analytic Philosophy* (New York, 1985), 269.

AHR Forum
Reply to David Hollinger

DAVID HARLAN

DAVID HOLLINGER THINKS I have become a groupie in the poststructuralist's metaphysical road show. Actually, I feel more like a bemused spectator watching a group of intellectual magicians. If I have been spending too much time reading Derrida and not enough time in the archives, it is because bewilderment is such a natural pleasure. But Hollinger thinks I have confused historical writing with storytelling. And that I have misrepresented his own writings. I will deal with the question of misrepresentation first and then take up the substantive issues that Hollinger raises. The most critical of these are: the belief in authorial presence; the feasibility of studying ideas in their original, historical context; and the danger of relativism, both interpretive and moral.

With regard to misrepresentation, here is how I described Hollinger's position: Hollinger "evidently believes that if an intellectual community expects to play an effective role in the larger culture it must organize itself around a set of shared commitments—commitments defined with enough rigor to 'delimit problems, direct research, and establish criteria of judgment.'"¹

It is true, as Hollinger points out, that in the article to which I was referring he likened American intellectual history to a "commons" that supports a variety of activities.² But it is also true that Hollinger thinks this diversity has gone too far, that he thinks we need to impose some order on the commons, and that in this article he tries to suggest the essential elements of that order: "As the term 'commons' implies, there are issues of common interest to scholars who work there. To bring these issues into focus and to make suggestions about their resolution might remind us that the commons should not be parceled out into small private lots. Yet this privatization of the commons is now a real danger: some scholars have become so complacent and autonomous in their work that what was once the most synthetic of American history's subdisciplines threatens to become the least."³

He then proceeds to identify a set of "basic commitments," acceptance of which he hopes might bring some discipline to the chaos on the commons. But,

¹ David Harlan, "Intellectual History and the Return of Literature," *AHR*, 94 (June 1989): n. 128.

² David Hollinger, "American Intellectual History: Some Issues for the 1980s," *In the American Province: Studies in the History and Historiography of Ideas* (Bloomington, Ind., 1985), 177.

³ Hollinger, "American Intellectual History," 181–82.

like so many others who have taken it upon themselves to define a community's "basic commitments," Hollinger also feels compelled to identify certain practices and practitioners that would no longer be unacceptable in the new order. He disparages the motives of those historians who have drawn upon recent work in literary criticism ("professional opportunism"), condemns what he calls "linguistic imperialism," and, in what Peter Novick recently criticized as a thinly veiled form of "nativism," he warns about potential defectors now residing among us—historians he suspects might be secretly preparing to join the ranks of the postmodernists ("among the potential volunteers for these new International Brigades, with battalions no doubt named for Saussure and Peirce, are scholars who think of themselves as 'intellectual historians'").⁴

I suggested in my article that Hollinger "would have us approach complex texts in a thoroughly instrumental fashion, using them to illuminate the discourse of which they are but manifestations." Hollinger thinks this is simple-minded, unfair, and factually wrong. True, in the essay in question, he did distinguish between two approaches, "one designed to enrich readings of particular texts," the other designed "to illuminate a larger discourse in which such a text may have been a part." And, "still fearful that some future David Harlan might miss the point," he did add that if the discipline of political philosophy did not already practice the first approach we would "need to create a discipline that did."⁵ But Hollinger's point was that the discipline of political philosophy *does* exist, that it already practices the first approach, and that we therefore need neither re-invent that discipline nor duplicate its efforts in our own discipline. Hollinger titled his article "Historians and the Discourse of Intellectuals." As the title clearly indicates, and as everyone who followed these debates knows perfectly well, Hollinger used this article to argue that intellectual historians should adopt "discourse and the communities formed around it" as their "central subjects." The article concerned, in Hollinger's own words, "the designation of communities of discourse as the [intellectual historians'] primary unit of study."⁶ In fact, this particular article was one of the most important and influential American statements of the then-fashionable idea that intellectual historians should become historians of "discourse." Finally, compare my statement that Hollinger "would have us approach complex texts in a thoroughly instrumental fashion, using them to illuminate the discourse of which they are but manifestations" with Hollinger's own assertion that the new discourse-oriented intellectual history would "use texts as instruments in order to better understand discourse."⁷ Readers can decide for themselves if my description of Hollinger's position constitutes either misrepresentation and/or "abuse."

⁴ Hollinger, "American Intellectual History," 188. For Novick's criticism of Hollinger, see Peter Novick, *That Noble Dream: The "Objectivity Question" and the American Historical Profession* (Cambridge, 1988), 606.

⁵ Harlan, "Intellectual History," n. 69; David Hollinger, "The Return of the Prodigal: The Persistence of Historical Knowing," *AHR*, 94 (June 1989): 618.

⁶ David Hollinger, "Historians and the Discourse of Intellectuals," in *New Directions in American Intellectual History*, John Higham and Paul Conkin, eds. (Baltimore, Md., 1979), 44, 45.

⁷ Hollinger, "Historians and the Discourse of Intellectuals," 52.

Now to the more substantive issues. First, with regard to the question of authorial presence. Hollinger writes, "I believe that authors remain present enough in texts to justify our listening for their voices." For all the genuine sincerity of his confession, Hollinger neither meets nor counters the arguments that have been brought to bear against authorial presence. But Hollinger writes better than he knows, for it is now beginning to look as though *belief* in the author's presence may be our best response. A number of writers, approaching the problem from a variety of disciplines, are now suggesting that, if we hope to make sense of any text, we must first *attribute* to it an author, an intending agent.⁸ This strikes me as an interesting and potentially fruitful formulation, one that we historians might do well to consider. I suspect that whatever progress we are able to make on the question of authorial presence will come along these or similar lines.

As for contextualism: again, for all the interesting things he has to say, Hollinger does not directly address the questions that have been raised. Rather than confronting them head-on, one by one, he mocks them or in some other way dismisses them. This is unfortunate for a number of reasons. First and most obviously because there is no conviction more widely spread or firmly held among professional historians than the conviction that we can and should at least *try* to place ideas, people, and events in their proper historical context. No one believes that we will fully succeed in this, of course, but most historians believe we should try. Indeed, this is what "doing history" means. For most historians, the contextualist imperative has become, as Hollinger says, the fundamental distinction between historical and non-historical studies. On the other hand—and this is the second reason why a point-by-point discussion of the contextualist issue would have been helpful—a minority of historians think we are moving in the opposite direction: not toward a hardening of the differences between historical and non-historical studies but toward an appreciation of their underlying similarities. What seems important from this perspective is not the unique importance of contextualism to historical studies but the *common* importance of narrative form to a variety of disciplines, both historical and non-historical. Third, it is precisely the attempt to rethink historical context, to define context in new and novel ways, to see historical objects in unexpected and sometimes startling historical settings—what the historian Peter Burke has called "the strategy of the surprising juxtaposition"—that makes the work of Louis Montrose, Stephen Greenblatt, Walter Benn Michaels, and the other New Historicists

⁸ See, for example, the psychologist Jerome Bruner, *Actual Minds, Possible Worlds* (Cambridge, Mass., 1986); the philosopher Daniel Dennett, *Brainstorms: Philosophical Essays on Mind and Psychology* (Cambridge, Mass., 1978), esp. "Intentional Systems," 3–22; and Dennett, *The Intentional Stance* (Cambridge, Mass., 1989), *passim*; the literary historians Steven Knapp and Walter Benn Michaels, "Against Theory," *Critical Inquiry*, 8 (Summer 1982): 723–42; and Knapp and Michaels, "Against Theory 2: Hermeneutics and Deconstruction," *Critical Inquiry*, 14 (Autumn 1987): 49–68. Bruner even went so far as to suggest that the need to envision authors and to attribute intentions to them is so deeply ingrained that it "constitutes a primitive category system in terms of which experience is organized, as least as primitive as [Kant's] category system of causality"; *Actual Minds*, 18–19.

so interesting and important.⁹ Fourth, a not-inconsiderable number of professional historians consider the theoretical questions that have been raised about contextualism interesting and important in and of themselves. For one reason or another, the problem of contextualism is of interest to all of us. One can only wish that David Hollinger, thoughtful and well-informed as he is, had addressed himself to the issue.

Indeed, Hollinger seems to have missed the central point of my article. On page 619, he writes that the article “would have been more challenging had [I] treated literature’s return not as a license for the old but as a charter for the new.” Perhaps, but the whole point of the article, its entire burden and thrust, was not to invent some new way of writing history but to reclaim an older way. I wanted to point out that, by discrediting contextualism, recent developments in literary criticism and the philosophy of language make it possible for intellectual historians to return to an *earlier* understanding of their discipline: intellectual history as a conversation with the dead about the things we value.¹⁰ Do recent theoretical developments really create such an opening? Can we actually go all the way with contemporary literary theory and somehow end up with traditional intellectual history? And, if we can, is this a kind of history we need right now? I *think* the answer to all three of these questions is yes, but they clearly need more careful scrutiny than they have yet received.

Finally, with regard to relativism. Hollinger is right: the return of literature does raise critical problems about interpretation, about meaning and about evaluation—in other words, about relativism. It is not difficult to understand why the deconstructionist’s seemingly joyous affirmation of a world without origin or truth reawakens, for many people, a dark undercurrent of dread. I say “reawakens” because many historians feel that they have heard all this before: the assertion that there are no objective facts, no universal truths, no permanent foundations; the insistence that there is only the endless multiplication of new perspectives, the relentless proliferation of ungovernable interpretations. As Ishmael put it, “The round world is but an empty cipher.” But to assert that there are no *universal* truths is not to assert that there are no *particular* truths; to assert that there is no single, absolute, human ideal that can be assimilated to politics is not to say there are no human ideals. This is an old and valuable lesson, a lesson that has helped us keep our balance in the past and could help us keep

⁹ Peter Burke, “Open the Curtain and See Their Puppets Play,” *London Review of Books* (January 5, 1989): 18. Interestingly enough, what the historian Peter Burke so admires in the “New” Historicist Stephen Greenblatt—his “strategy of the surprising juxtaposition”—is not unlike what the critic Kenneth Burke admired in Thorstein Veblen’s economic anthropology; he called it the strategy of “perspective by incongruity.” See Max Lerner’s introduction to *The Portable Veblen* (New York, 1948), 45. For a brilliant example of what might be called “The New Contextualism,” see George Cotkin, *William James: Public Philosopher* (forthcoming, Johns Hopkins University Press).

¹⁰ On the hope of actually speaking with the dead, see Stephen Greenblatt, *Shakespearean Negotiations: The Circulation of Social Energy in Renaissance England* (Berkeley, Calif., 1988), 1. Greenblatt is right, of course: we can never hope to speak with the dead; we can only interrogate those “textual traces” through which the dead make themselves present to the living.

it now. Epistemological modesty is probably a good thing in democratic societies, counseling, as it does, patience, tolerance, and mutual respect.¹¹

Forbearance—good word.

F. Scott Fitzgerald, *The Crack-Up*.

¹¹ Richard Rorty makes this point in his new book *Contingency, Irony, and Solidarity* (Cambridge, 1989), esp. 44–95. Thomas Haskell made the same point in a remarkably sensitive and thoughtful piece, “The Curious Persistence of Rights Talk in the ‘Age of Interpretation,’” *Journal of American History*, 74 (December 1987): 984–1012. But notice that Isaiah Berlin pointed all this out over thirty-five years ago. See his modernist interpretation of Machiavelli, originally delivered as an address in 1953. It has been most recently reprinted as “The Originality of Machiavelli” and can be found in Berlin, *Against the Current: Essays in the History of Ideas* (New York, 1980), 25–79, esp. 74–79. I am indebted to John P. Diggins for this reference, as for so many other insights and suggestions.

Recounting the Past: “Description,” Explanation, and Narrative in Historiography

ALLAN MEGILL

IT IS A RATHER WIDELY HELD OPINION among professional historians that the truly serious task of historiography, making it a contribution to knowledge and not a triviality, is the task of explanation. The opinion has roots in an objectivist myth that (as Peter Novick has recently shown) remains “powerful, and perhaps even dominant” within the historical profession.¹ In the objectivist view, the “description” of historical facts is unproblematic, as are the interpretive perspectives from which historians order these facts. Seconding the impact of professional historiography’s founding myth is the continuing authority of a somewhat outdated, but still in many ways influential, philosophy of science. How many of us have downgraded a student paper with the words, “This is mere description”? The critical judgment may well be sound, but the choice of language is unfortunate. One rightly criticizes a work of history for being mistaken or uninteresting, but there is no warrant for assuming that “description” is a lesser historiographical aim than explanation. The assumption mistakes the character of historiography’s contribution to knowledge; it misvalues narrative history in particular. The quality of a work is not adequately judged by its proportion of explanation to “mere description.” Rather, a proper judgment of quality concerns all the aims that a work sets for itself. These are of four intertwined kinds: interpretive, “descriptive,” explanatory, and argumentative or justificatory. It seems true by definition that every work of history embodies these aims. Different works embody them in differing degrees.

The argument I wish to make here is more theoretical than historical, even though I refer to empirical fact in articulating it. The aim is to counter

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¹ Peter Novick, *That Noble Dream: The “Objectivity Question” and the American Historical Profession* (Cambridge, 1988), 2.

professional orthodoxy. I investigate certain methodological contentions of François Furet and Fernand Braudel, addressing their validity and playing them off against Braudel's historiographic practice in *The Mediterranean and the Mediterranean World*. Furet and Braudel are used only as examples, for the intent is to go beyond the practice of specific historians to address some conceptual underpinnings of historiography generally. We should strive to be constrained neither by the particular "craft" exemplars that we admire (whose very merit threatens to imprison us), nor by unexamined assumptions in our milieu, nor by restrictive philosophies of science, but only by the limits of inquiry itself. Awareness of conceptual underpinnings helps us to see beyond what was and is being done to what might be done—to what is allowed by a discipline beyond the particular practices of its past and present exponents. This is why theory is important.

THE PRIVILEGED PLACE OF EXPLANATION in our academic culture is most clearly manifested in the theoretical and methodological literatures. I use the term "explanation" not in the broad sense of "to elucidate" or "to make clear" but in the sense customary in philosophical and social science circles, where in most contexts "to explain" something means to say what caused it. To explain something in the terminology used here is to answer the question, "Why?" taking that question in the sense of, "What caused it?"² Evidence of the privileged place of explanation, so defined, is to be found in the theoretical and methodological handbooks. Many announce explicitly their concern with explanation. Standard texts such as Arthur Stinchcombe's *Constructing Social Theories* are quite clear on this point.³ The handbooks are not always precise about what they mean by explanation, but usually the core if not the exclusive meaning is the answering of a causal question.⁴ Conversely, little has been written on "description," and in

² I am of course aware of the somewhat problematic status of the notion of cause in the positivist tradition in philosophy, but this is not an issue that need concern us here. For reservations about "cause," see Bertrand Russell's classic article, "On the Notion of Cause" (1912–13), in his *Mysticism and Logic, and Other Essays* (New York, 1918), 180–208. For a fastidious refusal to use directly the terms "cause" and "effect" combined with a constant invocation of these very terms, see Carl G. Hempel's equally classic "The Function of General Laws in History" (1942), in Patrick Gardiner, ed., *Theories of History* (New York, 1959), 344–56.

³ Among the works I have consulted are: Eugene J. Meehan, *Explanation in Social Science: A System Paradigm* (Homewood, Ill., 1968); Philippe Van Parijs, *Evolutionary Explanation in the Social Sciences: An Emerging Paradigm* (Totowa, N.J., 1981); Abraham Kaplan, *The Conduct of Inquiry: Methodology for Behavioral Science* (Scranton, Pa., 1964); Robert Borger and Frank Cioffi, eds., *Explanation in the Behavioral Sciences* (Cambridge, 1970); Patty Jo Watson, Steven A. LeBlanc, and Charles L. Redman, *Explanation in Archaeology: An Explicitly Scientific Approach* (New York, 1971); David Harvey, *Explanation in Geography* (London, 1969); Paul Kiparsky, *Explanation in Phonology* (Dordrecht, 1982); Willem Doise, *L'Explication en psychologie sociale* (Paris, 1982); Peter D. McClelland, *Causal Explanation and Model Building in History, Economics, and the New Economic History* (Ithaca, N.Y., 1975); Christopher Lloyd, *Explanation in Social History* (Oxford, 1986). On the explanatory function of social theories, see Arthur L. Stinchcombe, *Constructing Social Theories* (New York, 1968), vii, 5, and *passim*.

⁴ One reason for this relative unanimity in preoccupation and definition is that the theoretical and methodological literature remains deeply influenced by the philosophy of science of the 1940s, 1950s, and 1960s. Philosophers of science in that generation were overwhelmingly concerned with "explanation," which they viewed as the answering of the "why" question. See Carl G. Hempel, *Aspects of Scientific Explanation and Other Essays in the Philosophy of Science* (New York, 1965), 245, 344.

many of the social and behavioral sciences “descriptive” types of research are viewed with suspicion.⁵ To be sure, in recent years there has emerged a newer theoretical literature, emphasizing the interpretation and description of culture, that turns the focus of attention back to other aspects of understanding than the explanatory.⁶ But the older, “harder” methodology still occupies the commanding heights, the rhetorical high ground that the word “science” brings with it.⁷

To what extent, in their talking and thinking about historiography, do practicing historians share the view that explanation is the historian’s central task? Eschewing a research survey of impossible subtlety, let us move instead by introspection, informed by several salient examples of the “bias for explanation.”

In an essay in 1961, “Causation and the American Civil War,” Lee Benson made use of E. M. Forster’s distinction between “story” and “plot.” A story is “a narrative of events arranged in their time-sequence,” such as, “The king died and then the queen died.” A plot is “also a narrative of events, the emphasis falling on causality,” as in, “The king died and then the queen died of grief.” Benson went on to gloss the distinction in this way:

Using Forster’s criterion, we can define a historian as a plot-teller. Unlike the chronicler, the historian tries to solve the mystery of *why* human events occurred in a particular time-sequence. His ultimate goal is to uncover and illuminate the motives of human beings acting in particular situations, and, thus, help men to understand themselves. A historical account, therefore, necessarily takes this form: “Something happened and then something else happened *because . . .*” Put another way, the historian’s job is to explain human behavior over time.⁸

Or consider E. H. Carr’s assertion in *What Is History?* that “[t]he study of history is a study of causes” and his repeated characterization of a proper

and *passim*; Carl G. Hempel, *Philosophy of Natural Science* (Englewood Cliffs, N.J., 1966), 47, 49; Ernest Nagel, *The Structure of Science: Problems in the Logic of Scientific Explanation* (New York, 1961), 15–16; and Wolfgang Stegmüller, *Probleme und Resultate der Wissenschaftstheorie und Analytischen Philosophie*, Band I: *Wissenschaftliche Erklärung und Begründung* (Berlin, 1969), 77.

⁵ As Miriam Schapiro Grosf and Hyman Sardy have pointed out, *A Research Primer for the Social and Behavioral Sciences* (Orlando, Fla., 1985), 114, referring in particular to the case study type of research. Such research, they suggested, has a *preliminary* status: it may suggest hypotheses for further research and may provide “anecdotal evidence to illustrate more generalized findings” (112). One exceptional work, written from a neopositivist perspective, that does deal with description is C. Behan McCullagh, *Justifying Historical Descriptions* (Cambridge, 1984).

⁶ See Clifford Geertz, *The Interpretation of Cultures: Selected Essays* (New York, 1973), esp. chap. 1, “Thick Description: Toward an Interpretive Theory of Culture,” 3–30; Paul Rabinow and William M. Sullivan, eds., *Interpretive Social Science: A Reader* (Berkeley, Calif., 1979); Clifford Geertz, *Local Knowledge: Further Essays in Interpretive Anthropology* (New York, 1983); and Paul Rabinow and William M. Sullivan, eds., *Interpretive Social Science: A Second Look* (Berkeley, Calif., 1987).

⁷ For example, see the uneasy encounter of an anthropologist, committed to “description,” with a physicist, committed to explanatory generalizations, reported by Renato Rosaldo, “Where Objectivity Lies: The Rhetoric of Anthropology,” in John S. Nelson, Allan Megill, and Donald N. McCloskey, eds., *The Rhetoric of the Human Sciences: Language and Argument in Scholarship and Public Affairs* (Madison, Wis., 1987), 87.

⁸ Lee Benson, “Causation and the American Civil War,” in Benson, *Toward the Scientific Study of History: Selected Essays* (Philadelphia, 1972), 81–82. See E. M. Forster, *Aspects of the Novel* (New York, 1927), 47, 130, and *passim*.

historical account as one that gives the reader “a coherent sequence of cause and effect.”⁹

Consider finally David Hackett Fischer’s contention in *Historians’ Fallacies* that “history-writing is not story-telling but problem solving” and that historical narration is “a form of explanation.”¹⁰

If the historian-reader of this paper finds that he or she is in essential agreement with these three statements, I have established that the reader shares a bias for explanation. The statements by Benson and Carr assume that the essential connections in a historical account are causal. The revealing of causal connections is what I (and they) define as explanation. Fischer’s position is ambiguous, for his definition of explanation embraces elucidation generally, not just causal analysis. Nonetheless, his insistence that history is “not story-telling but problem solving” (a notion also advanced by Furet) seems to confirm the presence of an explanatory bias in my sense.¹¹

Insofar as they are numerous, readers who find no disagreement with these statements confirm the claim by the historian and theorist of history Paul Veyne that “[t]here is . . . a widespread idea that a historiography worthy of the name and truly scientific must pass from ‘narrative’ to ‘explanatory’ history.” They also confirm the philosopher Paul Ricoeur’s similar claim that in “history as a science . . . the explanatory form is made autonomous.”¹²

The bias for explanation in much contemporary thinking about knowledge needs itself to be historically understood.

First, much of our thought about science is excessively influenced, even at this late date, by the history of Newtonian physics. Earlier in our century, the philosophical school variously known as “logical empiricism” or “logical positivism”¹³ took an apparent fact about that history and converted it into a principle. Physical science in the eighteenth and nineteenth centuries was marked by attempts to extend Newtonian theory to ever more phenomena. To show how yet another range of phenomena could be derived from Newton’s laws was to give an explanation of them. The cutting edge of science, it seemed, was neither “descriptive” nor interpretive but explanatory. The payoff in physics did not come in the ordering of phenomena into descriptive types, as it did in natural history. Nor did it come in finding new ways of conceiving the physical

⁹ Edward Hallet Carr, *What Is History?* (1961; rpt. edn., New York, 1967), chap. 4, “Causation in History,” 113–43; quotes from 113 and 130; see also 111–12, 114, 138.

¹⁰ David Hackett Fischer, *Historians’ Fallacies: Toward a Logic of Historical Thought* (New York, 1970), xii, 131.

¹¹ In *Historians’ Fallacies*, xv, n. 1, Fischer defined explanation as follows: “To *explain* is merely to make plain, clear, or understandable some problem about past events, so that resultant knowledge will be useful in dealing with future problems.” Although Fischer did not notice it, there is a tension between the first and second clauses, since “useful[ness] in dealing with future problems” suggests knowledge of cause-effect relations, hence a causal conception of explanation.

¹² Paul Veyne, *Writing History*, trans. Mina Moore-Rinvoluceri (orig. French edn., 1971; Middletown, Conn., 1984), 305 n. 5; Paul Ricoeur, *Time and Narrative*, trans. Kathleen McLaughlin and David Pellauer, 3 vols. (orig. French edn., 1983–85; Chicago, 1984–88), 1: 175.

¹³ On these terms, see David Oldroyd, *The Arch of Knowledge: An Introductory Study of the History of the Philosophy and Methodology of Science* (New York, 1986), 231, 248. Despite its slight inaccuracy, I shall use the more familiar term, “logical positivism,” here.

world, for until the 1890s the Newtonian interpretive framework was thought to be beyond question.¹⁴

Second, within the context of the human sciences, a striking feature of secular, modernist academic culture has been its commitment to metaphors of verticality, most evident in Karl Marx and Sigmund Freud but not limited to those traditions. It is a common trope of modernist inquiry that things more or less directly observable are not the “real” reality. In this view, the task of inquiry is to get down to what is hidden—to “underlying” determinants, to the “fundamental” features of the situation. The perspective of inquirer and audience has a crucial role in determining what will be regarded as insightful rather than as mistaken or simply irrelevant. Metaphors of verticality tend to grant a privilege to the explanatory project. David Hume’s demonstration that we cannot observe causation underpins the view that explanation is *tieferliegend* (deeper) than description.¹⁵ When the philosopher of social science, Philippe Van Parijs, claimed that “any explanation assumes the operation of an *underlying mechanism*,” he unwittingly reported the presence of this same metaphor.¹⁶ Discussing Progressive social thought in America, Richard Hofstadter detected the assumption that “reality” is “hidden, neglected, and off-stage,” a similar trope with an identical function.¹⁷ When such metaphors are in place, the most striking insights will be those that claim to show how the “onstage” or “superstructural” things and events arise from previously invisible economic, sociological, or psychological conditions. These insights have an explanatory character, for they are answers to the question, “What caused it?”¹⁸

The cutting edge in a given discipline at a particular time may well be found in explanation. Base/superstructure metaphors are in no way contrary to the advance of knowledge, so long as they continue to produce new insights and so long as their heuristic, limited character is kept in view. But disciplines tend toward sclerotic self-satisfaction. Methodological rules articulated in one context

¹⁴ See Thomas L. Hankins, *Science and the Enlightenment* (Cambridge, 1985), 9, 20–21, 53; J. L. Heilbron, *Electricity in the Seventeenth and Eighteenth Centuries* (Berkeley, Calif., 1979), 6, 87 and following, 95 n. 47, 458 and following; and Christa Jungnickel and Russell McCormmach, *Intellectual Mastery of Nature: Theoretical Physics from Ohm to Einstein*, 2 vols. (Chicago, 1986), 1: xxiii and *passim*. On the classificatory impulse in natural history and elsewhere, see Hankins, *Science and the Enlightenment*, 113, 117, and *passim*; and Wolf Lepenies, *Das Ende der Naturgeschichte: Wandel kultureller Selbstverständlichkeiten in den Wissenschaften des 18. und 19. Jahrhunderts* (Munich, 1976), 34, 47–48, 93, 98–102, 122–24.

¹⁵ Stegmüller, *Wissenschaftliche Erklärung und Begründung*, 1. Kapitel, 2.b., “Erklärungen und Beschreibungen,” 77.

¹⁶ Van Parijs, *Evolutionary Explanation in Social Sciences*, 6.

¹⁷ Richard Hofstadter, *The Age of Reform from Bryan to F.D.R.* (New York, 1969), 199–200. See also Lionel Trilling on V. L. Parrington and Theodore Dreiser in Trilling, “Reality in America,” in his *The Liberal Imagination: Essays on Literature and Society* (New York, 1956), 3–21.

¹⁸ It seems plausible to suggest that thinkers less committed to the base/superstructure metaphor, or to other metaphors that envisage differentially visible realities, will be less committed to the explanatory project. One social science methodologist noted that most of Max Weber’s “theories” are actually “conceptual schemes and descriptions of ‘historical types’” (Jack P. Gibbs, *Sociological Theory Construction* [Hinsdale, Ill., 1972], 16). It may well be that there is a relation between Weber’s well-known suspicion of the base/superstructure metaphor and the fact that his great achievements seem much more to be “descriptive” and interpretive than explanatory.

are often inappropriately applied to other contexts. Interpretive frameworks all too often come to be seen as *die Sache selbst*.

Consider the bias for explanation as expressed in logical positivism. Of course, logical positivism has long been dead within philosophy. Killed by its own contradictions, it has given way to various neo- and post-positivisms. Nonetheless, the formulations of logical positivism are important for two reasons: these express emphatically and with precision notions less clearly expressed elsewhere, and, second, many non-philosophers, including some historians, still cling to logical positivist dicta of three or four decades ago and trot them out whenever they want to appear rigorous and methodological.¹⁹

In the first sentence of their widely cited paper, "Studies in the Logic of Explanation" (1948), Carl Hempel and Paul Oppenheim declared: "To explain the phenomena in the world of our experience, to answer the question 'why?' rather than only the question 'what?' is one of the foremost objectives of empirical science."²⁰ In a similar vein, Ernest Nagel asserted in *The Structure of Science*: "It is the desire for explanations which are at once systematic and controllable by factual evidence that generates science; and it is the organization and classification of knowledge on the basis of explanatory principles that is the distinctive goal of the sciences."²¹ As a final example, consider the following assertion by logical positivist historians, appearing in a work that aspired to set the agenda for social science history in the United States, the Social Science Research Council's "Bulletin 64": "The truly scientific function begins where the descriptive function stops. The scientific function involves not only identifying and describing temporal sequences; it also involves explaining them."²²

None of these authors denies that "description" is part of empirical science; such a denial would, of course, be anti-empirical. But, by the same token, all consider "explanation"—which they define essentially as I do here—to be "the truly scientific function." Given the rhetorical prestige that attaches to the term "science," we have no choice but to read these statements as manifestations of an explanatory bias.

Two mistaken prejudices supported—and continue to support—the bias. One is the prejudice for universality; the other is hermeneutic naïveté, or the belief in immaculate perception.

The prejudice for universality elevates explanation over "description" because in the logical positivist view "description" is tied to the merely particular, whereas explanation is seen as universalizable. In the immediate background to logical

¹⁹ The worst offender may well be economics: see Donald N. McCloskey, *The Rhetoric of Economics* (Madison, Wis., 1985), 7–8. But a survey in May 1987 by J. Morgan Kousser suggests that an "informal positivism," of an early Popper vintage, remains prevalent among historians: see J. Morgan Kousser, "The State of Social Science History in the Late 1980s," *Historical Methods: A Journal of Quantitative and Interdisciplinary History*, 22 (1989): 12–20, at 14.

²⁰ Carl G. Hempel and Paul Oppenheim, "Studies in the Logic of Explanation," in Joseph C. Pitt, ed., *Theories of Explanation* (Oxford, 1988), 9. On the importance of this article, see Ronald N. Giere, *Explaining Science: A Cognitive Approach* (Chicago, 1988), 28.

²¹ Ernest Nagel, *The Structure of Science: Problems in the Logic of Scientific Explanation* (New York, 1961), 4.

²² Social Science Research Council, Committee on Historiography, *The Social Sciences in Historical Study* [Bulletin 64] (New York, 1954), 86.

positivism stands the still remarkably influential opposition, first proposed by Wilhelm Windelband in 1894, between the “nomothetic” sciences, concerned with the search for general and invariable laws, and the “idiographic” historical disciplines, whose focus of attention is held to be particular entities.²³ At least in principle, Windelband accorded equal status to nomothetic and idiographic investigations: in his eyes, both were science (*Wissenschaft*). The logical positivists, in contrast, restricted the name and status of science to nomothetic investigations, to those fields producing, or claiming to produce, general laws.

Because they often confuse “general laws” with other kinds of generalizations, historians sometimes miss the full force of the idea that a field is scientific only if it produces general laws. By “generalization,” historians usually mean a broad statement that is nonetheless still tied to a particular historical context. In historians’ language, the following invented statement counts as a generalization (the question of whether or not the statement is correct does not concern us here): “As a result of the growth of towns and trade, feudalism gave way to incipient capitalism in late medieval and early modern Europe.” The “problem of generalization,” as historians conceive of it, is usually the problem of how to get from fragmentary and confusing data to such larger assertions.²⁴ But such assertions are not what the logical positivists, or Windelband before them, had in mind when they spoke of general laws. In “nomothetic” science, the desired generalizations *transcend* particular times and places, as in, for instance, this invented statement: “*Whenever*, within a feudal system, towns and trade begin to grow [we would likely find enumerated further conditions, along with statements concerning their interrelations], *then* feudalism gives way to capitalism.” In short, the generalizations in question are laws (which can be formulated as “if . . . then” statements), and assemblages of such laws brought together in theories.

The Windelbandian distinction between the particular and the general has often been equated with the distinction between “description” and explanation. Consider the following passage, which begins Hempel’s “Function of General Laws in History” (1942):

It is a rather widely held opinion that history, in contradistinction to the so-called physical sciences, is concerned with the description of particular events of the past rather than with the search for general laws which might govern those events. As a characterization of the type of problem in which some historians are mainly interested, this view probably can not be denied; as a statement of the theoretical function of general laws in scientific historical research, it is certainly unacceptable.²⁵

²³ Wilhelm Windelband, “History and Natural Science,” with an introductory note by Guy Oakes, *History and Theory*, 19 (1980): 169–85, esp. 175. See also Georg G. Iggers, *The German Conception of History: The National Tradition of Historical Thought from Herder to the Present*, rev. edn. (1968; rpt. edn., Middletown, Conn., 1983), 147–52.

²⁴ As in William O. Aydelotte’s classic paper, “Notes on the Problem of Historical Generalization,” in Social Science Research Council, *Generalization in the Writing of History: A Report of the Committee on Historical Analysis*, ed. Louis Gottschalk (Chicago, 1963), 145–77 (reprinted in Aydelotte, *Quantification in History* [Reading, Mass., 1971], 66–100).

²⁵ Hempel, “Function of General Laws in History,” 344–45.

As can be seen, at the same moment that he rejected Windelband's identification of historiography as idiographic, Hempel linked "description" to the particular. He did not argue for the linkage, but we can easily reconstruct why he believed that it was part of a view that "probably can not be denied." Consider again the two invented passages concerning the transition from feudalism to capitalism. The first has both a "what" and a "why" component. It is explanatory (or, more precisely, it claims to be explanatory), for it offers an account of why the transition from feudalism to capitalism took place. It is also "descriptive," for it says what was the case in late medieval and early modern Europe. But the second statement is different. It "describes" no reality. Rather, it states a set of hypotheses that are tied to no particular reality. Its relation is to concepts: feudal system, growth, cities, capitalism. When it is applied to a particular reality—say, Europe in the twelfth century or Lower Slobbovia in the twentieth—it has an explanatory payoff, at least if the audience in question accepts the stated laws as true and agrees that the concepts in question are appropriate to that reality. "Why was there a transition from feudalism to capitalism in Lower Slobbovia in the twentieth century? Well, it is because whenever . . ." And so we have a form of explanation that has a portability, a universalizability, that "description" cannot have.

Given the prejudice for universality, the result is a general elevation of explanation over "description." It is widely held in philosophy and in social science that only knowledge of the general or universal (as distinguished from the local or particular) is truly scientific; all else is inferior. The prejudice has roots in Greek thought, in Plato and (even more influentially for science) in Aristotle. In his *Metaphysics* and elsewhere, Aristotle contended that knowledge of universals is the highest form of knowledge.²⁶ In the *Poetics*, he noted the implication for history, observing that "poetry is something more philosophical and of graver import than history, since its statements are of the nature rather of universals, whereas those of history are singulars."²⁷ In the twentieth century, the universalizing commitment is still alive, although in modern thought it derives more directly from Hume and from Immanuel Kant than from Aristotle. Poetry has dropped out of the circle of universal knowledge, which is now restricted to mathematics, natural science, and social science insofar as it follows the natural science model thus projected.²⁸

²⁶ Aristotle, *Metaphysics*, trans. W. D. Ross, 982^a 20–25, in Aristotle, *Complete Works*, ed. Jonathan Barnes, 2 vols. (Princeton, N.J., 1984), 2: 1554; on the "commitment to the generic" in Greek thought generally, see Windelband, "History and Natural Science," 181. There is, let it be noted, another side to Aristotle, exemplified in the *Ethics* and *Rhetoric*, where emphasis lies on specific cases (of moral judgment or of persuasion). (See Stephen Toulmin, "The Recovery of Practical Philosophy," *American Scholar*, 57 [1988]: 337–52, at 339 and *passim*.) But modernism looks with disfavor on the ethical-rhetorical strand.

²⁷ Aristotle, *Poetics*, trans. Ingram Bywater, 1451^b 6–7, in Aristotle, "Rhetoric" and "Poetics," introd. Edward P. J. Corbett (New York, 1984), 235.

²⁸ On the idea of universality in modern thought, see, among others, Max Weber, "A Critique of Eduard Meyer's Methodological Views," in Weber, *The Methodology of the Social Sciences*, trans. Edward A. Shils and Henry A. Finch (Glencoe, Ill., 1949), 163 n. 30; Stanley Rosen, *Hermeneutics as Politics* (New York, 1987), 45, 95 and *passim*; and Richard W. Miller, *Fact and Method: Explanation, Confirmation and Reality in the Natural and the Social Sciences* (Princeton, N.J., 1987), 3–4.

Recently, the prejudice for universality has been challenged on many fronts. The interpretivist strand has already been mentioned (note 6). The revival of rhetoric launches another, related challenge.²⁹ Some methodologists of the social sciences have criticized excessive concern with universalizability.³⁰ Even in philosophy of science, the idea that science is preeminently a matter of finding “universal generalizations” has lately been questioned.³¹

Yet “interpretive social science” is still widely regarded as woolly headed, rhetoric is misdefined as deceit, and challenges to the universalizability criterion in social science methodology and philosophy of science are not yet sufficiently appreciated. To be sure, few historians ever committed themselves to the positivist program of theory construction that, for instance, the Social Science Research Council urged on them in the early 1950s.³² In fact, the divergent practices of sociologically oriented historians and historically oriented sociologists have helped to reinforce the sense that the aims of historians are divergent from those of theory constructors.³³ Even (perhaps especially) in social science history, the point now seems well understood.³⁴ Nonetheless, logical positivism’s “deductive-nomological” model of explanation (also known as the “covering law” model), which presupposes the pursuit of universal theory and seeks to explain

²⁹ Among the works important for this revival are Chaim Perelman and Lucie Olbrechts-Tyteca, *The New Rhetoric: A Treatise on Argumentation*, trans. John Wilkinson and Purcell Weaver (orig. French edn., 1958; Notre Dame, Ind., 1969); Stephen Toulmin, *The Uses of Argument* (Cambridge, 1958); and Thomas S. Kuhn, *The Structure of Scientific Revolutions*, 2d edn., enl. (1962; rpt. edn., Chicago, 1970).

³⁰ See Arthur L. Stinchcombe, *Theoretical Methods in Social History* (New York, 1978), esp. 1–4, 115–17.

³¹ See Giere, *Explaining Science*, 89–90, 102–04.

³² The authors of S.S.R.C. Bulletin 64 thought that “particular explanations of particular data . . . can contribute little to the cumulative growth of knowledge, unless used as the starting point for more systematic investigation and testing.” Translated, the S.S.R.C. authors are saying that a study of, for instance, the Third Reich is not in itself a serious contribution to “the cumulative growth of knowledge.” Only when such investigation yields a theory of, say, “fascist states” or “totalitarian political systems” has knowledge progressed. Consequently, the authors urged as a “general rule” that “problems should be defined and hypotheses developed as early in the analysis as possible,” and they rejected “*ad hoc* hypotheses, drawn upon only after the evidence has been selected.” The trouble with hypothesizing in the wake of, rather than before, serious examination of the evidence is that one’s account begins to wiggle and squiggle in response to the facts and so comes to be a “mere description” of those facts instead of generalizable theory. See *Social Sciences in Historical Study*, 27.

³³ The literature is large. But see, for example, the historian Olivier Zunz’s comments on recent comparative-historical sociology, which he found “does not provide any real alternative” to the universalizing perspective of an earlier generation of historical sociologists, despite claims by its practitioners to have abandoned that perspective (Olivier Zunz, “Toward a Dialogue with Historical Sociology” [review of Theda Skocpol, ed., *Vision and Method in Historical Sociology* (Cambridge, 1984)], *Social Science History*, 11 [1987]: 31–41, quote at 38–39). In the Skocpol anthology, the historian Lynn Hunt acutely noted that the work of the historian and sociologist Charles Tilly has been at its best when it is either predominantly historical or predominantly sociological, rather than “caught uncomfortably between the two” (269). See also Victoria E. Bonnell’s generally excellent “The Uses of Theory, Concepts and Comparison in Historical Sociology,” *Comparative Studies in Society and History*, 22 (1980): 156–73.

³⁴ Although there are exceptions. For example, one can detect, in Kousser’s conviction that “rational choice theory” can serve as the basis for a synthesis in the field of political history that would in no way narrow the discipline’s focus, a lingering attachment to older logical positivist hopes for synthesis through theory. (See J. Morgan Kousser, “Toward ‘Total Political History,’” California Institute of Technology, Division of Humanities and Social Sciences, *Social Science Working Paper*, 581 [September 1985; revised November 1986], i, 17.) Let us inscribe on our walls the following observation: all calls for “synthesis” are attempts to impose an interpretation.

particular realities in terms of theory, remains influential in such fields as economics, sociology, and political science. Beyond these fields, the logical positivist program still retains an aura of prestige, partly because of the decisive way in which logical positivism seized control of the rhetorically powerful term “science” and partly because of its well-justified insistence on clarity and explicitness in inquiry.³⁵

So the first mistaken reason for a general privileging of explanation over “description” is the prejudice for universality. The second, hermeneutic naïveté, leads not to the elevation of explanation but to the debasement of “description.” By hermeneutic naïveté, I mean the viewing of the historical account as if it were a “view from nowhere,” instead of—as it decidedly is—a view from some particular interpretive perspective. Modernist academic culture, particularly when it claims the prestige of science, tends to repress the interpretive dimension. Both Marx and Freud were notoriously prone to such repression, but their offense is far from unique. Once again, logical positivism provides an especially clear expression of a widely held view. Consider Hempel’s “Function of General Laws in History.” Many historian-readers will remember its centerpiece, the bursting of a car radiator. Hempel offered an explanation in a deductive-nomological form of the event, such that from certain initial and boundary conditions (for example, the bursting strength of the radiator metal, the temperature overnight) and from certain physical laws (for instance, concerning the freezing of water), the bursting of the car radiator can be deduced. The statement of initial and boundary conditions constitutes, of course, a “description.” Ironically, at the end of the essay, Hempel came to the proto-Kuhnian conclusion that the separation of “pure description” from “hypothetical generalization and theory-construction” is unwarranted.³⁶ Presumably, then, every “description” is already permeated by “theory,” as fact is by paradigm in Kuhn’s image of science. Yet, in dealing with the radiator example, Hempel failed to take account of his own conclusion. Instead, he proceeded as if “pure description” were indeed possible.

Hermeneutic naïveté is intertwined with the notion that “description” is intrinsically uninteresting. When the hermeneutic dimension is excluded, “description” gets reduced to data collection. On this point, positivism holds to a position that most historians will recognize as faulty. Yet, even among historians of some sophistication, there remains a tendency to underrate the force and scope of the hermeneutic insight that all perception is perspectival. Richard J. Bernstein has usefully (if schematically) distinguished between pre- and post-Heideggerian notions of the “hermeneutic circle.” In many standard characterizations, the circle runs between part and whole within the reality that the investigator seeks to understand. For instance, a historian or a textual critic will come to understand one sentence in a document in light of the document as a whole. But in its wider, post-Heideggerian sense, the circle runs between the

³⁵ See, on this last point, Lawrence Stone, “History and the Social Sciences in the Twentieth Century” (1976), in Stone, *The Past and the Present* (Boston, 1981), 16–18.

³⁶ Hempel, “Function of General Laws in History,” 356.

investigator and what is being investigated. The investigation will be prompted by the traditions, commitments, interests, and hopes of the investigator, which will affect what the investigator discovers. Conversely, the process of historical research and writing will change both the investigator and the audience—at least, it will do so if the inquiry is more than trivial.³⁷ To come to grips with the interpretive aspect of inquiry, one must make a reflexive move, looking at the way that the inquirer's point of view enters into the investigation. The long historiographic tradition that holds to the fiction of an objective narrator feigning to be silent before the truth of the past resists self-reflexive sensitivity.³⁸ The tradition goes along with an underrating of the "descriptive" project, which, as we shall see in relation to Braudel, is far more complex and interesting than a hermeneutically unaware perspective acknowledges.

IT WILL NOT HAVE ESCAPED THE READER'S ATTENTION that I have enclosed the term "description" in quotation marks, the intellectual equivalent of rubber gloves. Unfortunately, the word is tied almost umbilically to the notion of "mere description"—to the underrating of the project that it is meant to name. The project is the answering of the question, "*What* was the case?" as distinguished from the answering of the question, "*Why* was it the case?" (or "*What caused* it?") that is the hallmark of explanation. Given the infinite variety of perspectives from which a historical account can be written, both projects embody an infinite number of difficulties and possibilities.³⁹

Accordingly, a term not so suggestive of the mere copying of some external model is called for. Thus I prefer the term "recounting" to designate answers to, "What was the case?" Linked to the French *raconter*, the term encourages us to think of this historical answering on the model of the telling of a tale—in this case, a tale for the truth of which various arguments, documentary and otherwise, are made. There is clearly more than one way to tell a tale; by the same token, there are different ways of constructing the historical past. The new term helps us to appreciate that "description" is not a neutral preliminary to the *real* work of explanation, not mere data collection. It leaves us better able to see that the two cannot be given a differential importance in abstraction from the aims and audiences of particular historical works.

Those who miss the importance of recounting adopt (usually more or less unconsciously) one of two related positions. Either, while preserving a distinc-

³⁷ Richard J. Bernstein, *Beyond Objectivism and Relativism: Science, Hermeneutics, and Praxis* (Philadelphia, 1983), 135–36.

³⁸ See Robert Finlay, "The Refashioning of Martin Guerre," and Natalie Zemon Davis, "'On the Lame,'" *AHR*, 93 (June 1988): 553–71 and 572–603, for an exchange that highlights the need to attend carefully to the different voices and attitudes that manifest themselves in a work of history. Even historians aware of the hermeneutic tradition often resist the self-reflexive implications. Note, for example, Quentin Skinner's apparently unwitting reduction of post- to pre-Heideggerian hermeneutics in "Hermeneutics and the Role of History," *New Literary History*, 7 (1975–76): 209–32.

³⁹ As does the further argumentative or justificatory question: What grounds do we, author and audience, have for believing that such-and-such was the case, and that such-and-such is why it was the case? But I leave the justificatory question to the side in the present essay.

tion between recounting and explanation, they see recounting as uninteresting (as when it is taken as a mere preliminary to scientific knowledge), or they blend the two together but in such a way as to reduce recounting to explanation. In the two instances, the outcome is the same: an exclusion of recounting from the circle of valued knowledge.

The exclusion, moreover, is intimately bound up with questions concerning narrative and its validity. Narrative blends recounting and explanation. One of the effects of the bias for explanation and of the related bias for universality has been a debasement not just of "description" but of narrative. The celebrated "revival of narrative" has had to work against the still prevalent suspicion that "narrative history" is epistemologically and methodologically defective. When Lawrence Stone remarked that narrative "deals with the particular and specific rather than [with] the collective and statistical," it seems he was motivated in this assertion (which, as stated, is incorrect) by the uneasy thought that narrative is incapable of the theoretical universality that explanation by laws and theories promises, which would make it scientific. Thus narrative's alleged revival is shadowed by deeply held prejudices working against narrative—as they work against its primal component, recounting.⁴⁰

We can get at the questionable nature of these views by looking at their articulation by the *Annales* historian Furet, who dismissed both recounting and narrative for reasons closely connected to the philosophical and social scientific prejudices noted above. In "From Narrative History to Problem-oriented History" (1975), Furet discussed the advance of a new, analytical, conceptual, "problem-oriented" historiography and what he characterized as the "possibly definitive decline of narrative history."⁴¹ He approved of these developments, for narrative, he held, is logically and epistemologically flawed: "Narration's particular kind of logic—*post hoc, ergo propter hoc*—is no better suited to the new type of history than the equally traditional method of generalizing from the singular."⁴² Admittedly, as a *disabused* positivist, Furet denied that the transition from narrative history to "problem-oriented history" suffices to bring history into "the scientific domain of the demonstrable." Such a goal, he suggested, is probably unattainable, but at least the transition brings history closer to it.⁴³

To what extent is Furet's characterization of narrative adequate to reality?

⁴⁰ Lawrence Stone, "The Revival of Narrative: Reflections on a New Old History," *Past and Present*, 85 (November 1979): 3–24, quote at 3–4 (reprinted in Stone, *Past and the Present*, 74–96). Of course, even "the collective and statistical" does not rise to universality. "The collective and statistical" is itself "particular and specific." For *what* is being collected? Of *what* are the statistics gathered?

⁴¹ François Furet, "From Narrative History to Problem-oriented History," in Furet, *In the Workshop of History*, trans. Jonathan Mandelbaum (Chicago, 1982), 54–67; quote at 56.

⁴² Furet, "From Narrative History to Problem-oriented History," 57. See also *In the Workshop of History*, "Introduction," 8: "Traditional historical explanation obeys the logic of narrative. What comes first explains what follows."

⁴³ On the unattainability of the goal, see Furet, "From Narrative History to Problem-oriented History," 66–67. On the connection of Furet's preference for problem-oriented history to the prejudice for universality, see *In the Workshop of History*, "Introduction," 6–7 (the new history becomes, as a "form of knowledge," applicable to any and all societies). See also "From Narrative History to Problem-oriented History," 60, where historical demography's transformation of "historical individuals" into "interchangeable and measurable units" also points to the presence of the universalizability criterion.

Two points are of interest. First, like Stone, Furet alluded to narrative's supposed attachment to singulars, but, unlike Stone, he gave the attachment an explicitly negative cast by linking it to the empirical error of faulty generalization. The status of Furet's statement remains ambiguous, however, for he did not actually say (although his words appear to suggest) that narrative and generalizing from the singular have some special affinity for each other.

Much clearer is Furet's other assertion, that narrative follows the (il)logic of *post hoc, ergo propter hoc*. The same assertion has been made by some other writers as well—including the literary theorist Roland Barthes, whose own brief comments on the allegedly fallacious character of narrative help to gloss Furet's rather clipped statement. In an influential essay, "Introduction to the Structural Analysis of Narratives" (1966), Barthes held that narrative is characterized by a "'telescoping' of logic and temporality": "Everything suggests . . . that the mainspring of narrative is precisely the confusion of consecution and consequence, what comes *after* being read in narrative as what is *caused by*: in which case narrative would be a systematic application of the logical fallacy denounced by Scholasticism in the formula *post hoc, ergo propter hoc*."⁴⁴ Even though Barthes's statement may seem puzzling at first reading, the basic point is simple. Barthes is suggesting that narrative is a sequence of stated causes and effects. In short, he is making the same assertion about narrative that, above, we found Lee Benson and E. H. Carr making about historiography. By the same token, he is suggesting that narrative is essentially explanatory.

The causal/explanatory construal of narrative may be more familiar to some readers in the form given to it by the well-known American philosopher Morton White. In his *Foundations of Historical Knowledge* (1966), White contended that "a narrative consists primarily of singular explanatory statements," and that a history is "a logical conjunction of statements most of which are singular causal assertions." White distinguished history from chronicle, which is "a conjunction of noncausal singular statements." He then complicated matters by an explicit admission that a history may contain elements of chronicle and still be a history: this is why a historical narrative is only "primarily" causal or explanatory.⁴⁵ But he did not go on to consider what impact the structure of chronicle might have on that of historical narrative. Implicitly, he thought of chronicle as "mere chronicle," just as historical "description" tends to bear the guise of "mere description." History proper is causal/explanatory.

The Barthes and Furet formulation of this idea is easily subjected to empirical test, for it makes a clear statement about the extant things that we call narratives. Narrative, Barthes suggested, confuses "consecution" and "consequence," leading us to see whatever it is that comes "after" X as being "caused by" X. This will indeed be the case if narrative is a chain of stated causes and effects, A causing

⁴⁴ Roland Barthes, "Introduction to the Structural Analysis of Narratives," in his *Image, Music, Text*, trans. Stephen Heath (New York, 1977), 79–124, quote at 94. Barthes's statement is an intensification of Aristotle's assertion in the *Poetics* 1452^a 20 that "[t]here is a great difference between a thing happening *propter hoc* and *post hoc*."

⁴⁵ Morton White, *Foundations of Historical Knowledge* (New York, 1965), 4, 14, 222–25, and *passim*; quotes from 4, 223, 222.

B causing *C* causing *D*, and so on. If narratives actually do invite their readers to equate consecutiveness with consequence, *post hoc* with *propter hoc*, it follows that narrative does function as a chain of causes and effects. Further, if this is the case, narrative will be adequately understandable in terms of the category of explanation alone. Conversely, if we do not find the *post hoc*, *ergo propter hoc* fallacy prominent in actual narratives, this will suggest the need for precisely that attention to nonexplanatory elements in narrative that the recounting/explanation distinction encourages.

As it turns out, instances of causal-temporal confusion in narrative are fairly difficult to find. To be sure, in a perhaps unexpected narrative sphere, the cinema, Barthes's suggestion is illuminating, for it casts light on how viewers make sense of film action. When a camera shot shows one person pointing a gun, and the next shot shows another person falling to the ground and lying motionless, skilled viewers normally assume that the second person fell to the ground not only after the firing of the gun but also because of its firing. But the cinema is in some ways a special case of narrative, for there is usually no narrator's voice telling us the story; instead, the film feigns to show the story. In consequence, cinema seems to depend especially heavily for its coherence on viewer-inferred causal connections.⁴⁶ In written fiction, it is difficult to find instances of causal-temporal confusion in the absence of a narrator of a certain sort—one who, perhaps out of a stylistic commitment to parataxis, prefers to insinuate causal relations instead of stating them outright.⁴⁷ Written fiction thus makes clear to us that causal-temporal confusion is not an essential part of fictional narrative but results instead from the narrator's adoption of a particular style of narration.

As for historiography, it can be clearly shown, *contra* Furet, that causal-temporal confusion arises not from the act of narration itself but from lapses in argument or justification—the third aspect of the historical account, beyond recounting and explanation. Consider the following passage, from Nathan Rosenberg and L. E. Birdzell, Jr.'s *How the West Grew Rich*: "It is easy to imagine business enterprises formed among companions who learned to trust each other at war or at sea, for it happens often enough in our own times. (The generation which fought the American Civil War in their twenties, for example, invented the epitome of enterprises not based on kinship, the modern industrial corpo-

⁴⁶ As Seymour Chatman observed, "it requires special effort for films to assert a property or relation" ("What Novels Can Do That Films Can't [and Vice Versa]," in W. J. T. Mitchell, ed., *On Narrative* [Chicago, 1981]: 117–36, at 124). On the showing/telling distinction, see Wayne C. Booth, *The Rhetoric of Fiction*, 2d edn. (Chicago, 1983), 3–20. On the application of Barthes's dictum to cinema analysis, see Michèle Lagny, Marie-Claire Ropars, and Pierre Sorlin, *Générique des années 30* (Paris, 1986), 25–26.

⁴⁷ An example: "He had been chain-smoking for weeks. His gums bled at the slightest pressure from the tip of his tongue" (J. D. Salinger, "For Esmé—With Love and Squalor," in Salinger, *Nine Stories* [New York, 1983], 104). Note how unstable the "confusion" is: the addition of an explicit "because" ("Because he had been chain-smoking for weeks") would be enough to destroy it. On the distinction between paratactic style, which does not spell out ranks and relations, and hypotactic style, which does, see Richard A. Lanham, *Analyzing Prose* (New York, 1983), 33–52.

ration, in their forties.)”⁴⁸ In the second, parenthetical sentence, Rosenberg and Birdzell appear to be making two distinct statements. They tell us straightforwardly that the invention of the modern industrial corporation followed the Civil War experience. At the same time, they insinuate that the invention of the modern industrial corporation was caused by the Civil War experience. Lay readers may well find nothing wrong with this piggybacking of an insinuation on an assertion. But competently trained professional historians, when they encounter such a move, are likely to become suspicious and to ask for evidence. For example, how many of the inventors of the modern industrial corporation actually served in the Civil War? How close a connection can be drawn between such experience and their founding, two decades later, of corporations? What other factors might have prompted the development of corporations? The causal-temporal confusion in this text has nothing to do with the “particular logic” of narrative. It results from failure to adhere to a tacit rule in professional historiography against ambiguous assertion. One sees here an argumentative lapse, not the manifesting of an intrinsic property of narrative.

To sum up: narrative is not a scientifically disreputable application of the fallacy of *post hoc, ergo propter hoc*.⁴⁹ This is not surprising. What is surprising is that a view contradicted by the reading of almost any good narrative historian—Thucydides, for example—has been asserted without serious challenge. Perhaps this indicates the depth of the bias for explanation. The sociologist Arthur Stinchcombe suggested that “[a]s the professional tone has taken over history (from the praising and damning tone . . .), the normal linguistic effect is to make the narrative *appear* causal.”⁵⁰ Concluding that narrative is more than causal assertion, we are forced to attend to what is other than causal assertion in it. This means that we must attend to recounting.

FURET’S ATTEMPT TO DENY TO NARRATIVE HISTORY status as a legitimate form of knowledge production is closely connected to the distinction between narrative history and “problem-oriented history.” But Furet did not originate the distinction: it was proposed by Fernand Braudel in the same year, 1949, that the first edition of his *Mediterranean and the Mediterranean World* appeared. Thus the stakes are larger than Furet: they concern a scientific mythology that has long

⁴⁸ Nathan Rosenberg and L. E. Birdzell, Jr., *How the West Grew Rich: The Economic Transformation of the Industrial World* (New York, 1986), 125.

⁴⁹ After completing my analysis of Furet’s contention that historical narrative follows the logic of *post hoc, ergo propter hoc*, I discovered that the philosopher W. H. Dray has also attacked Furet on this and other points: see W. H. Dray, “Narrative versus Analysis in History,” in J. Margolis, M. Krausz, and R. M. Burian, eds., *Rationality, Relativism and the Human Sciences* (Dordrecht, 1986), 23–42, at 26 and following. Among other historians who, at one time or another, have linked narrative to a *post hoc, ergo propter hoc* logic are Lawrence Stone, *Social Change and Revolution in England, 1540–1640* (London, 1965), xxii; and Charles Tilly, *As Sociology Meets History* (New York, 1981), 90.

⁵⁰ Stinchcombe, *Theoretical Methods in Social History*, 13. Of course, as will be evident from my argument so far, tone is only part of the story. The explanatory bias derives more broadly from a certain view of science, from certain metaphors, from a concern in social science with pragmatic aims, and perhaps from other influences as well. Historiography does not exist in isolation from other intellectual and social practices.

shadowed (though fortunately never overpowered) the so-called *Annales* school. In a review of Charles-André Julien's book *Les voyages de découverte*, Braudel articulated the contrast between an *histoire-récit* that "too often hides the background of economic, social, and cultural facts" and an *histoire-problème* that "dives deeper [*plonge plus loin*] than events and men, a history grasped within the framework of a living problem or of a series of living problems clearly posed and to which everything that follows is subordinated, the joy of recounting [*raconter*] or of bringing the past back to life, the delights of making the great dead live again."⁵¹ How are we to characterize the *histoire-problème* that Braudel recommended? The answer is offered by J. H. Hexter in his wittily parodic article on Braudel (1972). *Histoire-problème* is history in which the asking of a "why" question—taking that question in the sense of, "What caused it?"—is uppermost in the historian's mind. In short, it is history that looks for explanations. As an example of *histoire-problème*, Hexter cited Edmund Morgan's article, "The Labor Problem at Jamestown, 1607–18," which sets out to answer the question why, in a colony that by 1611 was on the verge of extinction, the colonists were to be found "at their daily and usuall workes, bowling in the streetes" instead of raising the crops needed to keep them alive.⁵²

Hexter was forced to go to Morgan for an example because *The Mediterranean and the Mediterranean World* is not *histoire-problème*.

First, there is no single, overriding causal question that the work poses. For example, it does not ask the question, "What caused 'the Mediterranean world' to come into existence?" Of course, even to think of the question is to recognize the extreme difficulty of answering it. What about causal questions of a more specific sort? Hexter cited three instances: "Why did banditry flourish in the Mediterranean toward the end of the sixteenth century?" "What accounts for the considerable flood of Christian renegades into the service of the Turk and the Barbary states?" "Why did the Spanish ultimately expel the Moriscos?"⁵³ There are many more, but seen in relation to the work as a whole—1,375 pages in English translation—they play a relatively minor role. They appear intermittently. One will read for several pages—even, exceptionally, for a dozen pages or more—without encountering an answering (or even an asking) of a "why" question. Then a question and perhaps an answer will appear. But one has no sense that the explanation, whether offered or only called for, in any way determines the general shape of the text. The explanations seem embedded in something much larger that is not explanation. For example, in the first three sections of Part One, Chapter 1, which take up sixty pages in the English text, I find only three clear instances of explanation-seeking questions.⁵⁴ While Braudel

⁵¹ Fernand Braudel, "La double Faillite 'coloniale' de la France aux XV^e et XVI^e siècles" (review of Charles-André Julien, *Les voyages de découverte et les premiers établissements, XV^e et XVI^e siècles* [Paris, 1948]), *Annales: Economies, Sociétés, Civilisations*, 4 (1949): 451–56, quotes at 452, 453.

⁵² J. H. Hexter, "Fernand Braudel and the *Monde Braudellien* . . .," *Journal of Modern History*, 44 (1972): 480–539, at 535–38, discussing Edmund S. Morgan, "The Labor Problem at Jamestown, 1607–18," *AHR*, 76 (June 1971): 595–611.

⁵³ Hexter, "Fernand Braudel and the *Monde Braudellien*," 535.

⁵⁴ Fernand Braudel, *The Mediterranean and the Mediterranean World in the Age of Philip II*, trans. Siân Reynolds, 2 vols. (New York, 1966), 1: 77, 82, 83.

posed such questions somewhat more frequently elsewhere in the book, the early sections are not greatly atypical.⁵⁵

Second, it is a matter not simply of the intermittency with which Braudel offered explanations but also of the range of the explanations offered. The affinity between explanation and metaphors of verticality has already been noted. The metaphors are obviously present in Braudel's conceit that there exist three historical levels: the superficial, fast-moving, easily visible level of event; the more profound and slowly moving level of conjuncture; and the deepest geohistorical or structural level, which hardly moves at all and whose impact on human history is most easily missed.⁵⁶ Moreover, he accepted the challenge that the conceit offers to the historian, of explaining by connecting one level to another. His most explicit statement of this aim occurs at the end of the preface to the second edition;⁵⁷ it is also suggested in his review of Julien. And yet, as every serious commentator on *The Mediterranean* has observed, he failed to connect the different levels. Hexter noted that *histoire-problème* provides an answer to the problem "of bonding event, conjuncture, and structure."⁵⁸ But the answer is refused—to such a degree that the sociologist Claude Lefort, reviewing the work in 1952, saw in it a "fear of causality": "The condemnation of the causal relation leads [Braudel] into a pointillism that seems contrary to the sociological inspiration of the work."⁵⁹

Braudel himself seems to have recognized that *The Mediterranean* did not fit the *histoire-problème* mold. In the new introduction to Part Three written for the second edition, he suggested that recent research has made it possible for historians to choose from "two fairly well established 'chains'" in reconstructing the past—the chain of economic events and conjunctures, and the chain of political events. A fully explanatory history would presumably reduce one chain to the other. But he went on to assert that "[f]or us, there will always be two chains—not one."⁶⁰ In the same introduction, he referred to the "bedrock of history" that is geography, and then immediately suggested that "the metaphor

⁵⁵ A terminological note. Braudel frequently uses the word explanation in the broad sense of "to elucidate." He explicitly connects *expliquer* and *éclairer* in *La Méditerranée et le monde méditerranéen à l'époque de Philippe II* (Paris, 1949), 307 (the passage, in the introduction to Part Two, is omitted from the 2d edition). Accordingly, when Braudel uses "explanation," he does not always mean it in the sense in which the term is taken here. For one instance where it seems to mean "elucidation" without specific reference to causes, see *Mediterranean and the Mediterranean World*, preface to the 1st edition, 20: "This book is divided into three parts, each of which is itself an essay in general explanation."

⁵⁶ As Samuel Kinser pointed out, "Annaliste Paradigm? The Geohistorical Structuralism of Fernand Braudel," *AHR*, 86 (February 1981): 63–105, 83 and *passim*, Braudel changed his characterization of the first and second of these levels between the 1st and the 2d edition of *Mediterranean and the Mediterranean World*. Nor is Braudel necessarily committed to *three* levels in history. But these inconsistencies do not affect the basic point.

⁵⁷ Braudel, *Mediterranean and the Mediterranean World*, 1: 16.

⁵⁸ Hexter, "Fernand Braudel and the *Monde Braudellien*," 535.

⁵⁹ Claude Lefort, "Histoire et sociologie dans l'oeuvre de Fernand Braudel," *Cahiers internationaux de sociologie*, 13 (1952): 122–31; quote at 124. On the non-connection of levels in Braudel, see also Bernard Bailyn, "Braudel's Geohistory—A Reconsideration," *Journal of Economic History*, 11 (1951): 277–82, at 279; and H. Stuart Hughes, *The Obstructed Path: French Social Thought in the Years of Desperation* (New York, 1968), 58–59.

⁶⁰ Braudel, *Mediterranean and the Mediterranean World*, 2: 902.

of the hourglass, eternally reversible" would be a "fitting image" of the work.⁶¹ In short, he himself deconstructed the metaphor of verticality that accompanies his notion of *histoire-problème*.

To what genre, then, does *The Mediterranean and the Mediterranean World* belong if not to the professional genre of *histoire-problème*? Following Braudel, Hexter suggested that it is "total" or "global" history.⁶² The characterization begs to be filled in. In another important contribution to the Braudel literature, Hans Kellner showed that the totalizing aspirations (inevitably unfulfilled) of *The Mediterranean* help to identify it as an "anatomy" or "Menippean satire." In his authoritative account of this literary form (the best-known manifestation of which is perhaps Robert Burton's *Anatomy of Melancholy*), Northrop Frye noted some of its most striking features: it engages in "dissection or analysis"; it is "loose-jointed"; it manifests "violent dislocations"; and it is apt, through the "piling up [of] an enormous mass of erudition," to turn into an "encyclopedic farrago" to which "a magpie instinct to collect facts is not unrelated."⁶³ Even those who have only leafed through *The Mediterranean* should feel a sense of recognition. But the anatomy, as Frye also pointed out, is "a loose-jointed narrative form," manifesting "violent dislocations in the customary logic of narrative."⁶⁴ In short, *The Mediterranean and the Mediterranean World* is a work of narrative history.

It would be an understatement to say that *The Mediterranean* is not usually seen as narrative. But this is because "narrative" is usually taken to mean "the organization of material in a chronologically sequential order," to quote Lawrence Stone.⁶⁵ Stone follows in a venerable tradition. His definition of narrative has roots in the *Poetics*, where Aristotle gives primacy to plot (*muthos*) over the other elements making up a tragedy.⁶⁶ But if, as is usually done, we take "plot" to mean the sequence of actions within a work, the notion of plot focuses on only one aspect of narrative. "Action" implies an agent, and it also implies a setting within which action takes place. Accordingly, to make "chronologically sequential order" the defining feature of narrative is to engage in an arbitrary exclusion. To be sure, "traditional" historiography does tend to focus on action, and in consequence history has often been thought of as the story of actions—as the *historia rerum gestarum*. But we should not allow what is only an aspect of narrative to define narrative as a whole.

A century ago, Henry James called into question "the old-fashioned distinc-

⁶¹ Braudel, *Mediterranean and the Mediterranean World*, 2: 903.

⁶² On Braudel, *Mediterranean and the Mediterranean World*, 2: 1238, see Hexter, "Fernand Braudel and the *Monde Braudellien*," 530; see also 511.

⁶³ Hans Kellner, "Disorderly Conduct: Braudel's Mediterranean Satire (A Review of Reviews)," *History and Theory*, 18 (1979): 197–222, reprinted in Kellner, *Language and Historical Representation: Getting the Story Crooked* (Madison, Wis., 1989), 153–87; Northrop Frye, *Anatomy of Criticism: Four Essays* (Princeton, N.J., 1957), 308–14.

⁶⁴ Frye, *Anatomy of Criticism*, 309, 310.

⁶⁵ Stone, "Revival of Narrative," 3.

⁶⁶ Aristotle, *Poetics*, 1450" 2–17, in Aristotle, "*Rhetoric*" and "*Poetics*," 231. Even though, strictly speaking, the *Poetics* is about drama, not narrative, its influence far transcends such distinctions.

tion between the novel of character and the novel of incident.”⁶⁷ The difference between the two extremes is a matter of degree, not of kind. We can imagine a continuum, running from fast-paced plots of incident (“Kojak” or “Miami Vice”) to the novels of, say, Henry James. But the distinction between incident and character needs to be further broken down. Building on the Russian Formalist tradition, the theorist Seymour Chatman distinguished between action (carried out *by* an agent) and happening (an impingement *on* a character). One needs further to distinguish between character (which acts) and setting (which impinges). The interaction of the four elements produces the narrative. Two of the elements (action and happening) occur; two (character and setting) simply are. The first two we can call “events”; the last two, “existents.” (Of course, existents can come into being, but this is no denial of the distinction between the emergence of an existent, which falls under the heading of event, and the existent itself.) Emphasis on one of the four elements perforce limits the attention given to the others. One might express this idea by means of a formula:

$$(AH) \times (CS) = k$$

(action times happening [that is, “events”] times character times setting [that is, “existents”] equals a constant).⁶⁸ It is simply tradition, when it is not uninformed prejudice, that insists on identifying narrative history with actions and happenings, for characters and settings can also in principle serve as foci.

Accordingly, the crucial question to ask, in deciding whether a given work is best seen as an instance of narrative history, is not, “Is this text organized in a chronologically sequential order?” It is rather, “How prominent in the text are the *elements* of narrative?” In *The Mediterranean and the Mediterranean World*, they are prominent indeed, even though only Part Three, dealing with the “brilliant surface” constituted by political events, is chronologically ordered.⁶⁹ Succinctly put, *The Mediterranean and the Mediterranean World* is a work of narrative history that (except in Part Three) focuses not on events but on existents. Braudel turned the historical setting and the divisions and subdivisions of that setting into a vast collection of characters. These characters make up the single, all-embracing character that is “the Mediterranean and the Mediterranean world” itself.

Many of Braudel’s commentators have pointed to his penchant for personifying. In an early review, Lucien Febvre remarked that Braudel promoted the Mediterranean to “the dignity of a historical personage.” Hexter observed that Braudel populated the *longue durée* with “non-people persons—geographical entities, features of the terrain”; towns have intentions; the Mediterranean is a protagonist; even centuries are personalized. Kinser noted that Braudel treated

⁶⁷ Henry James, “The Art of Fiction” (1884, 1888), in James, *The Art of Criticism: Henry James on the Theory and the Practice of Fiction*, eds. William Veeder and Susan M. Griffin (Chicago, 1986), 174.

⁶⁸ See Seymour Chatman, *Story and Discourse: Narrative Structure in Fiction and Film* (Ithaca, N.Y., 1978), 19, 32, 34, 44–45, 96–145. My formula is an expansion of one proposed by Wallace Martin, *Recent Theories of Narrative* (Ithaca, N.Y., 1986), 117–18, which in turn is inspired by a rather different formula in Gérard Genette, *Narrative Discourse: An Essay in Method*, trans. Jane E. Lewin (Ithaca, N.Y., 1972), 166.

⁶⁹ Braudel, *Mediterranean and the Mediterranean World*, 2: 903.

space as “a human actor energetic and prompt to change costume.”⁷⁰ But we do not need to depend on the commentators, for Braudel himself was explicit about what he was doing. Consider the following passage, in the preface to the first edition: “Its character is complex, awkward, and unique. It cannot be contained within our measurements and classifications. No simple biography beginning with date of birth can be written of this sea; no simple narrative of how things happened would be appropriate to its history . . . So it will be no easy task to discover exactly what the historical character of the Mediterranean has been.”⁷¹

The Mediterranean and the Mediterranean World is best seen, then, as a vast character analysis, in which Braudel broke down “the Mediterranean,” which begins as an undifferentiated entity, into its constituent parts, with growing attention over the course of the book to the human processes that are carried out within this geohistorical space. By the time he was through, “the Mediterranean” had become a massively differentiated entity. This is what we learn: that “[t]he Mediterranean speaks with many voices; it is a sum of individual histories,” as Braudel wrote in the preface to the English edition (1972).⁷² *The Mediterranean* tells us what “the Mediterranean” was and, to some extent, what it still is. Braudel’s explanations are contributions to this end. The work is a vast recounting, into which explanations are stuck like pins into a pin cushion. It is likewise a vast narrative, though more an anatomizing narrative of character than a sequential narrative of action.

THE FORCE AND IMPLICATIONS of this essay’s distinction between recounting and explanation, and of its demonstration that Braudel’s *Mediterranean* is in fact a work of narrative, are likely to be misunderstood by many readers. Some will carry in their minds awareness of recent polemics, heavily marked by political commitments, concerning the desirability or undesirability of “narrative history.”⁷³ Some readers will be inclined, wrongly, to see my attack on positivism’s *a priori* privileging of explanation over “description” as, in some way, a rejection of the legitimacy and importance of historians’ explanatory efforts. Finally, some will misunderstand the nature of the distinctions that the essay poses. They are *conceptual* distinctions, aiming at clarity of thought about the historiographic enterprise. To say that a distinction can be made in thought is not to say that the elements thus distinguished will necessarily be clearly marked out in practice. In fact, the distinction between recounting and explanation is reader-constructed, but this is no denial of its reality, for the reader’s active involvement with the text is a necessary condition of understanding.

⁷⁰ Lucien Febvre, “Un livre qui grandit: *La Méditerranée et le monde méditerranéen à l’époque de Philippe II*,” *Revue historique*, 203 (1950): 218; Hexter, “Fernand Braudel and the *Monde Braudellien*,” 518–19; Kinser, “*Annaliste* Paradigm?” 67–68.

⁷¹ Braudel, *Mediterranean and the Mediterranean World*, 1: 17.

⁷² Braudel, *Mediterranean and the Mediterranean World*, 1: 13.

⁷³ For a brief account of the controversy, with relevant references, see Novick, *That Noble Dream*, 622–23; for a polemic against recent departures from “narrative history,” see Gertrude Himmelfarb, *The New History and the Old* (Cambridge, Mass., 1987); and the *AHR Forum* on the old history and the new, which follows this article.

As suggested at the beginning of this essay, recounting and explanation are but two of the four main tasks of historiography.

Recounting some aspect of historical reality—telling what was the case—is the first task. A work in which this aim dominates will inevitably be ordered in narrative form, as defined here—that is, historical actions, happenings, characters, and settings will play, in varying proportions, a prominent role in the text.

Following on this is the explaining of some aspect of historical reality. If explanation becomes the historian's main concern, the work may well begin, in its focus on connecting *explanans* and *explanandum*, to depart from a primarily narrative form (although narrative does accommodate explanations).

Third, the historian claims that his or her recountings and explanations are true: otherwise, we would conventionally regard this scholar as something other than a historian. Thus the historical account has a third aspect, that of argument or justification. If the historian deals primarily with “the sources,” the resulting account might well take the form of a commentary on, or analysis of, those texts. Alternatively, the purpose may be to justify a particular representation of the past against other possible representations, in which case the account would take on a primarily argumentative form. In both instances, the task of giving a narrative of the past would recede into the background.

Finally, the historian interprets the past—that is, necessarily, views the past from some present perspective. The perspective permeates all that the historian writes: we have access to no *regard de fin du monde*, and even if we did it would be but one interpretation among others, God's interpretation as distinguished from all the rest. Since the historical account is necessarily written from a present perspective, it is always concerned with the meaning of historical reality for us, now—even if, on an explicit level, it seeks to deny that it has any such concern.⁷⁴ To the extent that the concern with present meaning is dominant, the historian becomes not simply a historian but a social or intellectual critic as well. Here, too, the historical account might well cease to be primarily a narrative of past existents and events.

The limits of the categorial schema need to be kept in mind. The claim is not that the categories enable a complete analysis of works of history but only that they chart out important dimensions of the historiographic enterprise.

In illustration of the recounting/explanation distinction, which is the focus of the present article, consider the following sequence of statements excerpted from Burns, Lerner, and Meacham's freshman college history textbook, *Western Civilizations*—a work that, both in the usual definition and in the definition offered here, is an instance of narrative history:

- (1) In 1839, along with the other great powers, Britain had signed a treaty guaranteeing the neutrality of Belgium.
- (2) The Germans planned to attack France through Belgium.
- (3) [T]hey demanded of the Belgian government permission to send troops across its territory . . .

⁷⁴ For one such denial, see J. H. Hexter's charmingly naïve essay, “The Historian and His Day,” in Hexter, *Reappraisals in History* (Evanston, Ill., 1961), 1–13. The denial is of course central to the objectivist creed generally.

- (4) Belgium refused . . .
- (5) [T]he kaiser's legions began pouring across the frontier [anyway].
- (6) The British foreign secretary immediately went before Parliament and urged that his country rally to the defense of international law and the protection of small nations.
- (7) [T]he [British] cabinet sent an ultimatum to Berlin demanding that Germany respect Belgian neutrality, and that the Germans give a satisfactory reply by midnight.
- (8) The kaiser's ministers offered no answer save military necessity . . .
- (9) As the clock struck twelve, Great Britain and Germany were at war.⁷⁵

Each of the nine statements tells what was the case. But, taken collectively, they are more than just a sequence of recountings, for they offer an answer to the explanation-seeking question, "Why did Britain go to war against Germany?" Once the reader has passed through the recountings, he or she will be positioned to see that the text offers an explanation as well. (One of the difficulties that weaker students have in reading such textbooks lies in their failure to make this leap.)

Explanation is dependent on recountings. To explain, as defined here, is to give an answer to the question, "What caused it?" In order to ask the question, we need an "it." Thus the question, "What was the case?" is primal: it precedes the explanation-seeking question. But the explanations offered will themselves be recountings. Assume that an audience has been brought to an elementary understanding of, say, the French Revolution. The audience has been offered an outline of the revolution: that it began in France in 1789 with the meeting of the Estates General, that its first important symbolic event was the Oath of the Tennis Court, that the Estates General became, soon thereafter, the National Assembly, that this was followed by the storming of the Bastille, that there was a war and a Terror, and so on. As part of this recounting, explanations of historical existents and events will be offered. The explanations, once accepted by an audience as persuasive, will become part of its image of what was the case—part, that is, of what we might call a "representation" of the past. But images of what was the case always make possible further explanation-seeking questions. The further explanations, if accepted as persuasive, will enter into the image of what was the case and will make possible still more explanation-seeking questions.

Accordingly, what counts as an explanation in one rhetorical context may well count as a recounting in the next. The process is like the winning of land from the Zuider Zee. First, there is that part of the historical account that the audience—whatever audience it is, amateurs or the most "advanced" professional historians—simply accepts as what was the case, not (or not any longer) calling it into question. This is like land won from the Zuider Zee and now solidly under cultivation. Second, there is that part that the audience is inclined to ask further explanation-seeking questions about. This is like the present shoreline of the Zuider Zee. Persuasive answers to explanation-seeking questions are like

⁷⁵ Edward McNall Burns, Robert E. Lerner, and Standish Meacham, *Western Civilizations: Their History and Their Culture*, 10th edn. (New York, 1984), 927–28.

pumps and dikes that will turn this part, too, into dry land—into what is accepted as what was the case. There is next that part—not knowledge but nescience—that is too far from accepted recountings to permit explanation-seeking questions but which may become an object of explanation in the future. Here we have the center of the Zuider Zee, hidden beneath the waters. Finally, not to be forgotten, there is the wider society within which historians write. This is like the North Sea, whose storms may invade the dikes and inundate part or all of what had been won, with apparent security, for cultivation. When this happens, the old recountings, and the explanations subsidiary to them, will come to seem mistaken, or at least irrelevant to important concerns of the present. In response to the recession of their hitherto implicit persuasiveness, a revision of the past will come to be demanded.

Yet, for all the interweaving of recounting and explanation, the distinction between them is justified and important. Consider another passage from *Western Civilizations*:

The Coming of the Revolution

Faced with serious challenges to centralized power from the resurgent noble elites as well as popularly based political movements in the eighteenth century, only the ablest absolutist ruler, possessing in equal measure the talents of administrative ability and personal determination and vision, could hope to rule successfully. The French king, Louis XVI, possessed neither of these talents. Louis came to the throne in 1774 at the age of twenty. He was a well-intentioned but dull-witted and ineffectual monarch . . .

Conditions in France would have taxed the abilities of even the most talented king; for one with Louis XVI's personal shortcomings, the task was virtually insurmountable. Three factors, in particular, contributed to the breakdown that produced revolution.⁷⁶

Clearly, on one level, the passage offers us a recounting—a statement of what the authors believe was the case in France in the period preceding the French Revolution. But, on another level, they are beginning to give an explanation why the revolution occurred. While the distinction between recounting and explanation is not always clearly marked within the historical text, a clear marker is present here in the form of a “contrary-to-fact conditional,” or “counterfactual.” As philosophers have long known, a statement about causation implies a counterfactual statement. When a historian states that *C* caused (led to, occasioned, brought about) *E*, he or she is simultaneously implying that without *C* there would have been no *E*, all other things being equal.⁷⁷ In telling us that “only the ablest absolutist ruler . . . could hope to rule successfully,” the authors explicitly introduced the counterfactuality that is present at least implicitly in all

⁷⁶ Burns, Lerner, and Meacham, *Western Civilizations*, 674.

⁷⁷ There is a large literature. For a convenient introduction to philosophers' discussion of causation and counterfactuals, see Myles Brand, “Causality,” in Peter D. Asquith and Henry E. Kyburg, eds., *Current Research in Philosophy of Science: Proceedings of the P.S.A. Critical Research Problems Conference* (East Lansing, Mich., 1979), 252–81, at 264–69; see also the index entries for counterfactual conditionals in, among other studies, J. L. Mackie, *The Cement of the Universe: A Study of Causation* (Oxford, 1980); and Wesley C. Salmon, *Scientific Explanation and the Causal Structure of the World* (Princeton, N.J., 1984). Of more direct interest to historians is the chapter “Counterfactuals and the New Economic History,” in Jon Elster, *Logic and Society: Contradictions and Possible Worlds* (Chichester, Eng., 1978), 175–221.

explanation. Historians who remain unaware of how explanation, in its appeal to contrary-to-fact conditionals, differs from recounting tread on shaky epistemological ground.

Recounting and explanation do not subsist alone; rather, they fit within the fourfold matrix suggested above. Often in historiological discussion, a distinction is made between “narrative” and “analytic” history. But the narrative/analysis dichotomy is too crude to contribute much to understanding. Braudel’s *Mediterranean* shows that some narrative is heavily analytic—that is, it engages in the differentiation of hitherto undifferentiated entities. Conversely, much analysis proceeds in (conventionally) narrative form, following “chronologically sequential order”: a model instance is Marx’s *Class Struggles in France*.⁷⁸ The term “narrative” is used confusedly in contemporary theoretical discussion, although the confusions cannot be unpacked here. As for analysis, it takes place in quite different intellectual contexts, established by the four tasks of recounting, explanation, justification, and interpretation.

We have seen already that analysis can occur in the context of recounting. It occurs also in the context of explanation: thus Marx’s detailed analysis of the class structure of French society in 1848 aims at explaining why the French revolution of 1848 turned out as it did. Finally, it occurs in the contexts of justification and interpretation, although when these dimensions are dominant the writer-inquirer is likely to be viewed as a textual critic or social critic respectively, rather than as a historian.

Related to the narrative/analysis distinction is the distinction between “narrative” and “problem-oriented” history that Furet developed out of Braudel. In “From Narrative History to Problem-oriented History,” Furet seems to imagine a breaking free of the latter from the former. In the introduction to *In the Workshop of History*, he complained that the British historian of France, Richard Cobb, “turns history into a laboratory for a purely existential preference.” Hating “ideas” and “intellectualism,” Cobb transforms the quest for knowledge “into a passion for novelistic narrative.” Lacking “intellectual constructs,” he is a social historian for whom “only individuals exist.” His narrative is guided by a sympathy for the “life” of the period he describes. Sympathy, which replaces “the explicitly formulated question” as a guide to research, “belongs to the realm of affection, of ideology, or of the two combined.” Thus history à la Cobb “remains purely emotional,” failing to maintain “cultural distance between the observer and the observed.” The product of such a history is “erudition”—not, we are given to understand, the true seriousness of a “problem-oriented history that builds its data explicitly on the basis of conceptually developed questions.”⁷⁹

Yet, in his neopositivist commitment to a universalizable (or at least a comparable) history that will supersede the current “proliferation of histories,”⁸⁰

⁷⁸ Karl Marx, *The Class Struggles in France: 1848 to 1850*, in Karl Marx, *Surveys from Exile*, ed. David Fernbach [Marx, *Political Writings*, vol. 2] (New York, 1974), 35–142.

⁷⁹ Furet, *In the Workshop of History*, “Introduction,” 13–20. The final quotation is from a version of Furet’s introduction that appeared as “Beyond the *Annales*,” *Journal of Modern History*, 55 (1983): 389–410, quote at 409; in the book, Furet recommended “an intellectualist history that builds” (20).

⁸⁰ Furet, *In the Workshop of History*, 16.

Furet swept under the rug the fact that, on nicely “conceptual” grounds, explanation cannot be autonomous. Moreover, like many in the positivist tradition, he has forgotten that the explanatory theories that he wants historians to deploy presuppose particular interpretive standpoints that the theories themselves do not bring to light. Recountings (and explanations as well) must be carried out from some place, for some motive. The interpretive dimension is thus inescapable. The first words of *The Mediterranean and the Mediterranean World* are telling on this point: “I have loved the Mediterranean with passion.”⁸¹ Braudel’s words are as “affective” as anything in Cobb and his history as “erudite” as anything Cobb has written. These facts might be taken as excluding *The Mediterranean* from the true ranks of disciplinary history. In a review in 1953, Bernard Bailyn criticized the book for being “an exhausting treadmill,” ruined by the fact that “[t]here was no central problem Braudel wished to examine,” painfully lacking in “proper historical questions.”⁸² But precisely at issue is what constitutes a “proper historical question.”⁸³ To focus on explanation alone is to exclude this issue: and yet it perpetually returns.

To say that explanation presupposes recounting is to say that it presupposes a presentation of narrative elements. But historiography is a collective enterprise, and it is quite possible for an individual historian to forgo, in greater or lesser degree, the telling of a narrative that is already largely known. Indeed, such a procedure often seems necessary if historical knowledge is to advance. To the extent that a basic narrative is not told but presupposed, the elements of narrative will tend to fade into the background. In such cases, there is a genuine departure from narrative history. Thus, in rejecting the narrative/analysis contrast, I am not making the essentially empty gesture of declaring all history to be narrative history.

The historian who is clearest on this matter is Alexis de Tocqueville. Consider the beginning of *The Old Régime and the French Revolution*: “The book that lies before you is not at all a history of the Revolution, for that history has been written with such success that I cannot dream of doing it again; instead, it is a study on the Revolution.”⁸⁴ Tocqueville is true to his word. Time and again in *The Old Régime and the French Revolution*, he referred to historical events and existents without recounting them in detail, relying instead on the reader’s knowledge of them. His relative neglect of recounting freed him to move

⁸¹ Braudel, *Mediterranean and the Mediterranean World*, 1: 17.

⁸² Bailyn, “Braudel’s Geohistory—A Reconsideration,” 279, 281.

⁸³ Compare Bernard Bailyn, “The Challenge of Modern Historiography,” *AHR*, 87 (February 1982): 1–24, at 5: “Braudel’s *Méditerranée* . . . should be known . . . for its ahistorical structure, which drains the life out of history. For the essence and drama of history lie precisely in the active and continuous relationship between the *underlying conditions* [my italics] that set the boundaries of human existence and the everyday problems with which people consciously struggle.” How does Bailyn *know* that this is where the “essence and drama” of history lie?

⁸⁴ Alexis de Tocqueville, *L’ancien Régime et la révolution*, in Tocqueville, *Oeuvres, papiers et correspondances*, ed. J.-P. Mayer (Paris, 1951–), 2, 1: 69: “Le livre que je publie en ce moment n’est point une histoire de la Révolution, histoire qui a été faite avec trop d’éclat pour que je songe à la refaire; c’est une étude sur cette Révolution.” In the standard English-language edition, *The Old Régime and the French Revolution*, trans. Stuart Gilbert (Garden City, N.Y., 1955), vii, Tocqueville’s point is slightly obscured.

forward on the three remaining fronts. He addressed head-on the explanation-seeking question, "What caused the revolution?"⁸⁵ He argued explicitly against that representation of the revolution that saw it as essentially an attack on religious and political authority. He was likewise explicit about the interpretive dimension of the book and hence about the social criticism that it offers. As he observed, "I have never quite lost sight of present-day France." Thus, among other things, he sought to throw into relief "those virtues so vital to a nation" that he found absent from the contemporary scene but present earlier.⁸⁶

Much of the hostility, often quite visceral, that in recent years has been directed against departures from "narrative history" seems actually to reflect an uneasiness about the knowledge-expanding capacity of an academic discipline. There is in some quarters a longing for the repetition of old pieties, the careful burnishing of myth. Physics, psychology, and even sociology seem largely protected from such expectations, but historiography, it appears, is not. It may seem tempting, in the face of such challenges, to take refuge in a sophisticated neopositivism that would stress the specifically explanatory task of historiography over its other tasks—for we are inclined to see explanation as somehow insulated from issues of value.

The bias for explanation extends well beyond those historians influenced by positivist theory and methodology. For example, in a widely discussed essay, a neo-Hegelian intellectual historian told us that intellectual history "must address the issue of explanation, of why certain meanings arise, persist, and collapse at particular times and in specific sociocultural situations."⁸⁷ Of course, this particular exercise in explanation, and explanation generally, is certainly part of what intellectual historians do. But to privilege the explanatory dimension is to put into the background the framework of assumptions that every explanatory project presupposes. These assumptions derive from the historian's own traditions, commitments, interests, and experience, which finally cannot be historicized, cannot be subordinated to an objective, authoritative representation of history. The conservative critics of historiography are correct: history *is* about values. Historians qua historians, given the largely unreflexive character of their discipline, do not seem especially well equipped to deal with this fact.

Nonetheless, historians can at least know what they are doing when they are contributing to knowledge. It is not simply that they explain, as some contend. On the contrary, they first of all recount, in delight or fascination or horror or

⁸⁵ See especially the string of "why" questions that he poses in his foreword, and his final chapter, "How, Given the Facts Set Forth in the Preceding Chapters, the Revolution Was a Foregone Conclusion" (Tocqueville, *Old Régime and the French Revolution*, x, 203–11).

⁸⁶ Tocqueville, *Old Régime and the French Revolution*, xii. I write in the margins of Furet's highly intelligent and illuminating analysis of Tocqueville's book: François Furet, "De Tocqueville and the Problem of the French Revolution," in Furet, *Interpreting the French Revolution*, trans. Elborg Forster (Cambridge, 1981), 132–63. Characteristically, Furet tended to conflate the interpretive dimension of Tocqueville's project, concerned with "the meaning of his own time," with the task of articulating "explanatory theory" (132–33; see also 159–60). But, as I have argued, these are two distinct (though related) projects.

⁸⁷ John E. Toews, "Intellectual History after the Linguistic Turn: The Autonomy of Meaning and the Irreducibility of Experience," *AHR*, 92 (October 1987): 879–907; quote at 882. Compare David Harlan, "Intellectual History and the Return of Literature," *AHR*, 94 (June 1989): 581–609.

resignation. Upon recountings, explanations arise.⁸⁸ Recountings and explanations presuppose an interpretive perspective, and, in the best histories, they modify and enrich such a perspective. The articulation of perspectives is a contribution to knowledge that historians too often overlook or view with discomfort. To these tasks, justification stands as a *sine qua non*. Thus we see the legitimacy of narrative and, more generally, the legitimacy of forms of knowledge often deprecated in social science. The argument is not for the maxim “anything goes.” It is rather for a critical pluralism, for standards of evaluation appropriate to the forms of knowledge being sought. It is a contribution to rigor, not a detraction from it.

⁸⁸ See, again, Herodotus, *The History*, trans. David Grene (Chicago, 1987); and Thucydides, *History of the Peloponnesian War*, trans. Rex Warner (Harmondsworth, Eng., 1954). Herodotus was more inclined to become caught up in the sheer fascination of what he tells, while Thucydides leaned more toward explaining things. But both historians did both.

AHR Forum: The Old History and the New

The following essays were delivered on December 27, 1988, at the opening session of the Annual Meeting of the American Historical Association, Cincinnati, Ohio. The session was organized by members of the Program Committee with the aim of examining some of the current intellectual cleavages in the American historical profession. Although the essays appear here substantially as they were delivered, each author has had an opportunity to make small improvements and corrections. James H. Billington, the Librarian of Congress, served as chair at the session and introduced the participants—The Editor

The Bureaucratization of History

THEODORE S. HAMEROW

WHEN IN 1984 THE AMERICAN HISTORICAL ASSOCIATION commemorated the centenary of its founding, the prevailing mood was appropriately festive and celebratory. There were conferences, symposia, colloquia, and round tables tracing the way in which the discipline had advanced in the last hundred years and describing the newly developed fields, newly discovered techniques, and newly gained insights. The profession looked back with understandable pride at the remarkable progress it had made, at the transformation of a gathering of a handful of scholars into a vast organization embracing thousands. Was there not good reason for self-satisfaction and self-congratulation? History as an organized branch of learning had unquestionably come a long way.

But now that the tumult and the shouting have died, it may not be inappropriate to look back once again, less jubilantly perhaps, more soberly, at the way in which history has changed in the course of a century. To those who founded the American Historical Association, professionalization had meant above all a more scholarly, more scientific, more fruitful discipline. They believed that the organization and institutionalization of history would transform what had been a scholarly avocation or hobby into a disciplined, systematic quest for truths as indisputable as those of physics or chemistry.

"These years from 1880 to 1900," wrote Charles M. Andrews, who had lived through that exciting era, "were a time of great awakening in the American historical world, as effective in its way as was the corresponding awakening taking place in the field of the natural sciences. It was a time of exhilaration and almost religious fervor among the younger scholars, who saw new spheres of opportunity opening before them and entered on the quest with the zeal of explorers making new discoveries or of crusaders advancing to new conquests." As he recalled forty years later the sense of mission inspiring those who had set out at the end of the nineteenth century to transform historical study, Andrews still felt that here was "a true renaissance, in which the conception and treatment of history, under the inspiring leadership of men who saw visions and dreamed dreams, rose above the level of mere schoolmastering and became creative. This was the springtime of the historical movement in America."¹

To what extent has the development of the historical profession lived up to the expectations of the founders of the American Historical Association? To a considerable extent, no doubt. If they could attend the current meeting of the organization they had created, they would be enormously impressed. They would marvel at the vast increase in membership, at the hundred or more scholarly sessions held in the space of three days, at the dozens of affiliated learned organizations, at the breakfasts and the luncheons, the receptions and the social hours, the sheer diversity of specialties, techniques, purposes, and interests. To someone who had been present when it all started a hundred years ago, when there were only fifteen professors and five assistant professors of history in all the colleges and universities in the United States, a week in Cincinnati in 1988 would be an extraordinary experience. For there can be no doubt that the content and structure of historical learning have become fundamentally transformed.

Why then not join in the celebration of professionalization? Why be a wet blanket? It is because a price has been paid for the achievement of this progress, a price which may well be worth the purchase but which has strained and diminished the resources of the purchaser. And yet the reshaping of historical learning must also be seen as part of a broad cultural process by which scholarship in the course of the twentieth century became bureaucratized and rigidified in institutions of higher education. It reflects the same forces that have led to the transformation of the humanities as well as the social and the natural sciences into professional academic disciplines.

In other words, the formation of the American Historical Association was not the cause but the result, the manifestation of a basic change in the organization of historical learning. A similar change can be seen in virtually every other field of study. The physicist and the chemist moved out of the cellar or attic, where they had been conducting their scientific experiments, into an elaborate, expensive laboratory maintained by some university, institute, or foundation. The philosopher and the literary critic left the pulpit or Grub Street to find a

¹ Charles M. Andrews, "These Forty Years," *AHR*, 30 (January 1925): 233-34, 236.

more secure livelihood teaching in the classroom. And the result has been that scholarship as a whole became divided into numerous separate, independent, and distinct branches of learning. Simply put, it became bureaucratized.

This process involved several important changes in the organization of research and scholarship. The first was the introduction of a clearly defined program of training, usually involving a prolonged period of study at an institution of higher education, the completion of which became a requirement for admission to the profession. The acquisition of technical proficiency was as a rule attested by the conferral of the doctoral degree, which began to serve as a hallmark of scholarly competence. Those who failed to earn the Ph.D., however dedicated or talented, came to be regarded as less than fully qualified members of the discipline. A growing distinction thus developed between professionals and amateurs. The former, sacerdotal in outlook and superior in attitude, regarded the latter with disdain. They in turn felt resentment toward the professionals who increasingly dominated a field of study the amateurs had once ruled. But, in the end, the bureaucratization of learning inevitably meant the exclusion of those who did not possess proper academic credentials.

Another result was specialization and fragmentation. The professional historian, like the professional biologist or the professional economist, was expected to achieve mastery of one of the subdivisions of the discipline to which his or her scholarly activity would thereafter be directed and confined. Historians soon learned to meet, confer, discuss, argue, and agree or disagree primarily with other scholars in their specialties. They learned to restrict their research and writing to some recondite, unexplored area in a particular field such as sex and family in Restoration England or the disobedience and rebelliousness of labor in early German industrialization. For it would be highly risky to encroach on a neighboring but independent area like sex and family in Georgian England or the disobedience and rebelliousness of labor under the Weimar Republic. It would invite criticism for not being sufficiently familiar with the "latest scholarship," for not being close enough to the "cutting edge," wherever that might be. Hence, for prudent scholars, the course of wisdom became to stick to their own specialties. There they would be secure against the scholarly watchdogs guarding the other specialties.

The bureaucratization of learning led in turn to the growing estrangement between the broad educated public and the world of scholarship. In the nineteenth century, it was still common for leading scientists, economists, philosophers, and linguists to give lectures before large, appreciative audiences of interested amateurs. Even the most eminent or abstruse scholar would often address nonprofessional groups organized to promote the knowledge, understanding, and cultural improvement of their members. A close connection was thus maintained between theoretical investigation and practical experience, between research and community, between study and life. But the transformation of amorphous scholarly disciplines into distinct professional castes had the effect of weakening this tie between learning and society. Intellectual activity became increasingly isolated from the world of affairs. Scholars ceased to be

concerned with communicating their findings to an interested though inexpert public. Instead, the accepted means of publishing the results of research became the scholarly journal and the specialized monograph. The fact that outsiders were now no longer able to understand the findings of organized learning came to be regarded not as a sign of cultural isolation but as a proof of intellectual profundity. Those scholars who still tried to bridge the widening gap between abstract thought and everyday existence were generally dismissed as journalists, popularizers, or hacks. Learning became rigidified and hieratic.

The effect of this transformation of scholarship was especially profound in history. Here was a discipline in which the connection between practical experience and contemplative study had been traditionally very close. For over two thousand years, longer than in any other field of learning, historical research had been conducted not by professional scholars but by self-taught amateurs who had spent most of their lives in politics, warfare, theology, bureaucracy, journalism, or literature. What attracted them to the study of the past was a spontaneous curiosity, an instinctive interest in how the world had become what it was, how society had changed and grown with the passage of time. They did not bring to the discipline technical expertise or systematic methodology but exuberance, enthusiasm, excitement, and joy in learning. Even more important, their historical writing was informed by a rich practical experience in the world of affairs. Theirs was no cloistered scholarship fusty with archival dust, smelling of the lamp and leather binding. It was lively and vibrant, rousing and compelling. It had the breath of life.

The virtues of the amateur historians should not be exaggerated, to be sure. Their strengths were also their weaknesses. They displayed in the writing of history the same passions and prejudices, the same interests and predispositions that they revealed in everyday life. Some of them were jingoes and chauvinists celebrating the virtues of their class, their nation, or their race. Some were moralists and sermonizers preaching that divine Providence had decreed the eternal submission of the lower classes to the established order. And some were simply tattletales and gossipmongers specializing in amusing anecdotes or scandalous revelations about those in high places. Yet, in the hands of the best nonprofessional scholars, in the hands of a Gibbon or a Macauley, a Voltaire or a Tocqueville, historiography acquired a sharpness of insight and an understanding of character that only familiarity with everyday practical affairs can provide. They helped shape the golden age of historical writing.

This long millennial tradition of amateur history, subjective and undisciplined yet spontaneous and exciting, came to an end in the twentieth century. Little by little, those who had been trained, tested, doctored, and diplomaed began to displace those who could bring to the study of the past nothing but talent, enthusiasm, and love for the subject. For about forty years, the struggle raged, sometimes obscured by the appearance of mutual respect or at least tolerance but always bitter and unrelenting. On one side were the nonprofessionals, once the undisputed masters of the field, now fighting a losing rear-guard action against what they considered dry-as-dust pedantry that would stifle the creative

spirit with footnotes and bibliographies. On the other side stood the triumphant professionals, convinced that they represented the wave of the future, confident of their scholarly superiority, determined to transform the discipline through a more precise methodology and more critical analysis. Between two so diametrically opposed views of historical learning, no compromise was possible.

The struggle, increasingly uneven and futile, continued until World War II. But the rapid growth in the number of historians after 1945 led to the final victory of the professionals. The new generation of scholars, trained in a graduate school, armed with the doctoral degree, and conversant with the reconditeness of monographic research, had little patience with the unorganized, unsystematic, hit-and-miss approach of the amateurs. The result was the expulsion of the latter from the discipline. History became more exact but less eloquent, more scientific but less exciting. The amateurs continued to write and to publish, they continued to attract readers and to earn royalties. They often achieved a popularity most professionals could not approach. But that did not matter. They were denied the cachet of scholarly respectability because they had no Ph.D., they had no monographic publication, they did not as a rule attend learned conferences or symposia, they were not even in most cases members of the American Historical Association. How then could they be real historians? They were forced to seek consolation in their book sales and royalty statements. Yet they were not the only ones to suffer a deprivation. The discipline as a whole became poorer through the loss of their talent, exuberance, experience, and insight.

Even more serious was the departure of the nonprofessional readers of history. For the discipline had from its beginning attracted a wide public, drawn from all walks of life, which sought in the past a sense of collective identity or a guide to collective action. To some of them, history was a means of establishing a link between what was and what had been, between the individual and the community. It provided an insight into human destiny from a vantage point loftier than ordinary, everyday existence. It helped create a feeling of shared experience between generations, between ancestors and descendants, between the living and the dead. It seemed to satisfy a profound instinctive yearning to see human society as the outcome not of some haphazard aggregate of random contingencies but of a long, slow process of organic growth. It gave direction to the search for roots.

That had not been the only attraction of history, however. To the intelligent reading public, a knowledge of the past seemed the key to understanding the future. Thus the historian was also in a sense an oracle, a prophet. He or she could help the community make more intelligent choices, adopt more fruitful policies. Is it any wonder that the lectures and books of prominent historians enjoyed such wide popularity? They appeared to furnish a bright light amid the darkness enshrouding man's fate. The historian, it must be admitted, could not really live up to all the expectations of society regarding the predictive value of history. The inscrutability of the human condition is too profound, the illumination provided by historical learning too feeble to make possible more than a

few informed guesses, conjectures, and speculations. It would be deceptive or self-deceptive to pretend otherwise. And yet what other guide is there? The historians who once prophesied so confidently about the future of humankind were no doubt guilty of the sin of pride, intellectual pride. But they were also attempting to communicate to society at large what they believed to be the indisputable lessons of their discipline.

To be sure, not all of those who read and listened to history were seeking a sense of identity or an insight into the future. Some were simply interested in a good story, all the more fascinating because it was true. They were carried along by the sweep of a great national epic, by the account of the peopling of a continent, of the rise of a few backwater colonies to the position of a world power, of a tragic, bloody war between North and South, and of the struggle to create a freer and more just social order. In other words, the sheer drama of history attracted wide audiences for the same aesthetic reason that literature or music or art attracted them. And, to historians of a hundred years ago, that reason seemed entirely valid. They did not look down on those who sought inspiration or excitement in the past, and they did not dismiss those who provided it as storytellers or gossipmongers. The tie between scholar and community was still close.

The professionalization of knowledge has now destroyed that tie. Historians have ceased to write for ordinary readers and begun to write for other historians. They have become more specialized, more adept, more abstruse. Their claim to recognition no longer rests on the ability to explain to society where it has come from and where it is going but on their success in finding a new subdivision or approach or methodology or technique within the rigid confines of the discipline. History has become more detached, more aloof from the interests of the community that had once looked to it for direction and guidance. It has largely ceased to be part of the popular culture. By a curious paradox, at the same time that the field was becoming more accessible and egalitarian, it was also becoming more exclusive and elitist.

Yet the professionals have been able to avoid paying a penalty for their cultural isolation. They have managed to survive despite their abandonment of the vital function of communal instruction and edification. Their salvation has been higher education. They have retreated from the marketplace to the classroom; they have ceased to be bards and oracles and become schoolmasters. Secure behind the walls of the campus, they no longer need to worry about public favor, they no longer need to adapt to changing popular tastes and fashions. They are free to concentrate on an intellectual discourse with other professionals and on the pursuit of academic politics. How does the number of history majors compare with the number of majors in sociology, economics, or political science? Will the dean smile on the history department or, instead, favor one of its competitors? Which colleague will get the next major appointment, the next endowed chair, the next award for distinction? Such are the concerns of a discipline that has withdrawn from the hazards of literature and prophecy to become part of the college curriculum.

Perhaps all this is too harsh. Perhaps it depreciates the progress history has made as a result of professionalization. Let us by all means acknowledge that the scope of the discipline has widened in our century, that it has begun to examine issues and problems, groups and communities that traditional historiography had largely ignored. The broader range of scholarly research, its richer insight and deeper sympathy, may well be worth the cost of cultural isolation and intellectual esotericism. In any case, there can be no going back to an earlier and more innocent age when the historian still played the augur and the poet, the orator and the priest. *Tempora mutantur*. Yet, while celebrating the advances made by historical scholarship, we should also count the losses suffered as the price of those advances. We have become not only stronger but a little weaker, not only richer but a little poorer.

And one last point. Admittedly, the profession could not turn back the clock even if it tried. The changes taking place in the last hundred years have been too profound, too fundamental to be reversed. They are part of a broad cultural transformation leading to the institutionalization and bureaucratization of all knowledge. What has happened in history only reflects what has happened in every other branch of scholarship. We must therefore learn to understand and accept the basic alteration that the discipline has undergone. Yet, even as we do so, we should not ignore the adverse effects of institutionalization and professionalization, of specialization and fragmentation. Not all the results of the growing expertness of historical learning have been beneficial.

Once we recognize that, we may be able to mitigate some of the harsher consequences of the bureaucratization of history. Perhaps historians can learn to narrow a little the gap between professional and amateur, between academic and free-lancer. Perhaps they can do a bit more to impart to the public a sense of collective identity. Perhaps they can even offer the community some cautious guidance based on historical experience. To be sure, they must resist the temptation of transforming their sympathies and predispositions into immutable laws of history. They must avoid the intellectual hubris that so often characterized the scholarship of the past. Yet, by drawing closer to vital public interests and concerns, they can still enrich not only the discipline of which they are students but the society of which they are members.

AHR Forum
Some Reflections on the New History

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WHEN THIS SUBJECT, THE NEW HISTORY, WAS FIRST PROPOSED to me, I thought I understood what it meant. I am no longer so sure. The varieties of new history have proliferated so rapidly, the rhetoric and rationale have become so bold, and the entire discipline has gone so far beyond the old "new history" that one is tempted to speak of the "new new history."

Twenty years ago, one of the great masters of the new history, Le Roy Ladurie, could confidently pronounce, "Tomorrow's historian will have to be able to programme a computer in order to survive"; "History that is not quantifiable cannot claim to be scientific."¹ Six years after issuing those dicta, two years after reprinting them, and, ironically, four years before they appeared in an English edition, Le Roy Ladurie published another work that is as remote from that kind of quantitative, scientific history as is imaginable. His *Montaillou* was immediately acclaimed the classic of *mentalité* history.

And so it has been with other varieties of the new history. While one school of neo-Marxists is rewriting the old Marxist history in the light of a "humanistic" Marx, another school is revising the revisionists by reaffirming a rigorously deterministic and materialistic Marx.² One group of social historians insists that the essence of the historical enterprise lies in elucidating "Big Structures, Large Processes, Huge Comparisons,"³ while another insists that the true reality of history can be found only in the small, intimate details of "everyday life." *Annalists* who once confidently asserted the primacy of "long term" forces in history—demography, geography, ecology—have had to mute their claims in deference not only to their colleagues of the *mentalité* school but also to those who have rediscovered the importance of "mere" political events.⁴ Social historians devoted to the study of the working class are being criticized by feminist

¹ Emmanuel Le Roy Ladurie, *The Territory of the Historian*, trans. Sian and Ben Reynolds (Chicago, 1979), 6, 15.

² In England, the quarrel is between E. P. Thompson and Perry Anderson; on the Continent, between the disciples of Habermas and those of Althusser.

³ This is the title of a book by Charles Tilly (New York, 1984).

⁴ François Furet, introduction to *In the Workshop of History*, trans. Jonathan Mandelbaum (1981; Chicago, 1984). Jacques Julliard calls for a "retour de politique," although not quite the traditional kind of political history. ("La politique," in Jacques Le Goff and Pierre Nora, eds., *Faire de l'histoire* [Paris, 1974], 2: 229 and following.)

historians for being insufficiently attentive to gender.⁵ And now we are witnessing the emergence of the newest of the new histories, deconstructionism, which threatens to deconstruct much of the new history together with the old. (It has been said that when ideas die in France, they are reborn in America; one might add that when they are past their prime in other disciplines, they are belatedly adopted by historians.)

Like Hegel's "moments of history," these schools of history do not succeed each other in a tidy fashion. There is a limit, after all, to how often and how quickly historians can "retool" themselves (as one put it to me). Thus the older coexists, more or less uneasily, with the newer. Occasionally, an older new historian, dissatisfied with the state of his discipline, makes a conciliatory gesture toward the old history, such as calling for a "revival of narrative."⁶ But that much-quoted pronouncement is less significant than it sounds. For the kind of narrative that is proposed—the "narration of a single event," as exemplified in Le Roy Ladurie's *Montaillou*—is nothing like narrative history in the traditional sense, which is not confined to a single event but rather connects in a narrative sequence a series of events over a significant span of time. (Fernand Braudel's *Mediterranean* has also been assimilated into the genre of narrative history; but this makes the concept so inclusive as to be meaningless.)⁷

If the call for a revival of narrative is more rhetorical than substantive, it does testify to a widespread recognition on the part of new historians that their discipline lacks form and structure. This is the refrain that runs through a recent volume of essays by social historians, with one after another complaining of the absence of any synthesizing or unifying theme, the diversity and disparity of subjects and methods, the gap between macrohistory and microhistory. One contributor sums up this sense of malaise by suggesting that the challenge confronting the social historian is "putting more history into social history."⁸

I was first made aware of this problem, the problem of putting more history into social history, several years ago when I heard a historian describe the study he was working on: an "in-depth analysis" as he put it, of the life and work of the inhabitants of a New England town in the late eighteenth century. He explained that his colleagues were doing comparable studies of other towns and that their collective effort would constitute a "total history" of that time and place. I asked him what bearing that total history would have on what I, admittedly not an expert in American history, regard as the most momentous event of that time and place (indeed, one of the most momentous events in all of modern

⁵ For instance, Joan Wallach Scott, *Gender and the Politics of History* (New York, 1988), chaps. 3 and 4.

⁶ Lawrence Stone, *The Past and the Present* (London, 1981), 74 and following.

⁷ Paul Ricoeur, *Time and Narrative*, trans. Kathleen McLaughlin and David Pellauer (Chicago, 1984), 224–25, 230. This "new" form of narrative, as applied to Ladurie or Natalie Davis, is sometimes referred to by the deconstructionists as "narrativist."

⁸ William B. Taylor, "Between Global Process and Local Knowledge: An Inquiry into Early Latin American Social History, 1500–1900," in *Reliving the Past: The Worlds of Social History*, ed. Olivier Zunz (Chapel Hill, N.C., 1985), 121. One is reminded of Pieter Geyl's comment about Toynbee: "C'est magnifique, mais ce n'est pas l'histoire." (Quoted by Walter A. McDougall, "'Mais ce n'est pas l'histoire!': Some thoughts on Toynbee, McNeill, and the Rest of Us," *Journal of Modern History*, 58 [March 1986]: 26.)

history)—the founding of the United States. He replied that from his subjects and sources he could not “get to” that event, but he assured me that it was not as important as I took it to be, that what was important was the ordinary life of ordinary people. When I wrote about this episode, adding my own observation that even ordinary people (perhaps most of all, ordinary people) had been profoundly affected in the most ordinary aspects of their lives by the founding of this republic, I was rebuked for taking so elitist a view of history.⁹

My essay on this subject was written only five years ago. Since then, more and more social historians have acknowledged that something is missing from social history, that more history has to be put into social history. Marxists have pointed out that the preoccupation with daily life obscures the importance of class interests and the class struggle.¹⁰ A Latin American historian complains about the insufficient attention paid to the role of the state in establishing the economic and social hegemony of the ruling class.¹¹ An American social historian reports that his colleagues “downplay” the Civil War: in spite of their sophistication about the nature of conflict in general, this crucial conflict “still eludes social historians.”¹² An eminent *Annaliste*, reviewing the many volumes of that influential journal, is struck by the virtual absence of any discussion of the major event in French history, the French Revolution.¹³ And an English social historian remarks upon the apparent inability of the *Annaliste* school to deal with “the great formative political events in a country’s history”—the Risorgimento, for example, as well as the French Revolution.¹⁴ (It is interesting that the bicentenary of the French Revolution has inspired a revival of interest in political affairs on the part of historians who once spurned them as “mere events.”)

One recent exercise in self-criticism deserves special attention, if only because it comes from an eminent new historian. In his presidential address to the American Historical Association two years ago, Carl Degler described the “splintering,” “fragmenting,” and “disarray” that have afflicted American history and have become a common complaint in the profession.¹⁵ As a result of the

⁹ Gertrude Himmelfarb, “‘History with the Politics Left Out’” (1984), reprinted in *The New History and the Old* (Cambridge, 1987).

¹⁰ Elizabeth Fox-Genovese and Eugene D. Genovese, “The Political Crisis of Social History: A Marxian Perspective,” *Journal of Social History*, 10 (Winter 1976): 213–15; Tony Judt, “A Clown in Regal Purple: Social History and the Historians,” *History Workshop: A Journal of Socialist Historians*, 7 (Spring 1979): 66–94.

¹¹ Taylor, “Between Global Process and Local Knowledge,” 166.

¹² Zunz, *Reliving the Past*, 76.

¹³ Furet, *In the Workshop of History*, 11.

¹⁴ Eric Hobsbawm, report of conference on “The Impact of the *Annales* School on the Social Sciences,” *Review*, 1 (Winter–Spring 1978): 65.

¹⁵ Carl N. Degler, “In Pursuit of an American Dream,” *AHR*, 92 (February 1987): 1–2. See also Bernard Bailyn, “The Challenge of Modern Historiography,” *AHR*, 87 (February 1982): 1–24; John Higham, epilogue to a new edition of *History: Professional Scholarship in America* (Baltimore, Md., 1983); Alan Brinkley, “Writing the History of Contemporary America: Dilemmas and Challenges,” *Daedalus*, 113 (Summer 1984).

This loss of coherence is a common complaint in other disciplines, and for much the same reasons, because they are all trying to be all things to all factions. Recently, within only a few weeks, one read that anthropology was suffering from “grave inner uncertainties, amounting almost to a sort of epistemological hypochondria”; a report on the annual conference of sociology headlined “Sociology Branches Out But Is Left in Splinters”; and an account of a convention of archaeologists treated to the latest version of “critical archaeology”—“guerrilla archaeology,” *Times Literary Supplement* (August

enormous expansion of social history, the subjects of that history—blacks, women, Chicanos, American Indians, immigrants, families, cities—are all “clamoring for inclusion in a historical framework that once had no place for them.” Unable to integrate them into anything that might be called “the history of the United States,” we find ourselves with a history that is “disjointed and incoherent,” that lacks “central themes or a framework,” and that does not try to answer any “significant questions” of the kind raised by previous historians. To rectify this situation, to encompass all these new subjects and at the same time restore some coherence to American history, Degler proposed that American historians focus upon the theme of “national identity” and address the question: “What does it mean to be an American, that is, a citizen of the United States?”¹⁶

That proposal may be more agreeable to an old historian than to a new one. The ideas of nationality and citizenship have been criticized by the new history precisely because they presume a unity that is thought to be spurious, because they impose a political identity upon nonpolitical entities (notably race, gender, and class), and because they perpetuate the “hegemony” of the established political elite over all the groups suppressed and oppressed by the old history—women, blacks, Chicanos, etc. It is difficult, moreover, to see how the subjects of the new history can be accommodated in any single framework, let alone a national and political one. For what they are all “clamoring for” is not a place on the periphery of history—that they always had—but at the center, and not intermittently but permanently. What they are all seeking is to be “mainstreamed” into American history, to be made, as Degler has said of women’s history, an “integral part” of history.¹⁷ But how can all these groups, each cherishing its uniqueness and its claim to sovereign attention, be mainstreamed into a single, coherent, integrated history?

And how can they all agree on any significant question, much less a common answer? If no subject is more important than any other, how can any question be more significant than any other? Who is to say, the new historian may object, that the question of what it means to be a citizen of the United States is more central to the historical enterprise than what it means to be a woman in the United States, or a black in the United States—or, perhaps, a homosexual or a homeless person? Who is to say that “national identity” is more important than race, gender, or class—or, for that matter, that class is more important than gender? A recent review in the *Times Literary Supplement* criticized a book on British social history because “the discussion of sexual matters and gender differences tends to

26–September 1, 1988); *New York Times* (August 30, 1988); *Washington Times* (September 15, 1988). See also “The ‘New’ Archaeology,” *Intercollegiate Review*, 21 (Fall 1986); Ernest Gellner, “The Stakes in Anthropology,” *The American Scholar*, 57 (Winter 1988); Louis A. Sass, “Anthropology’s Native Problems: Revisionism in the Field,” *Harper’s Magazine* (May 1986); Henry Kariel, “Praxis: The Repressed Potential of Political Science,” *Polity*, 20 (Winter 1988).

¹⁶ Degler, “In Pursuit,” 1–2. Degler raised the same issue of national identity in his presidential address to the Organization of American Historians in 1980 (*Journal of American History*, 67 [June 1980]).

¹⁷ Carl Degler, “Women and the Family,” in *The Past Before Us: Contemporary Historical Writing in the United States*, ed. Michael Kammen (Ithaca, N.Y., 1980), 326.

take second place to class and income differentials.”¹⁸ There is, after all, only one first place. In the democratic ethos of the new history, no subject, no theme, no question wants to take second place to any other. At the very least, each regards itself as equal to every other. If some turn out to be more equal than others, it is certainly not nationality or citizenship that enjoys that favored status in the new history.

All historians, new and old, have something new to worry about—not only the fragmentation of history but the deconstruction of history—and not only on the part of avowed deconstructionists but on the part of social historians who unwittingly contribute to the same result.

In literary criticism, deconstruction means the liberation of the text from all the constraints that have traditionally given it meaning, starting with the intentions of the author—the “authorial voice,” as is said. The author, according to this view, speaks with no more authority than the reader or the critic. To the extent to which the author (putative author, one might say) is presumed to exercise any authority over the text, that authority is “authoritarian.” (The play on words is deliberate, and deliberately pejorative; that illicit authority has been described as “tyrannical,” “reactionary,” “imperialistic,” “fascistic.”) The deconstructionist also liberates the text from the tyranny of what is called “context”—the context of events, ideas, conventions, which informed the text not only for the author but for contemporary readers. “Nothing outside the text,” Jacques Derrida has proclaimed. And the text itself is said to be “indeterminate” because language does not reflect or correspond to reality; there is no correspondence between language and fact, between words and things. Indeed, there are no facts apart from language—which is why “facts,” in deconstructionist discourse, normally appears in quotation marks. Moreover, language itself is “duplicitous,” “cryptic”; it has to be “decoded” before it can convey any meaning. And since there is no single correct code, no reading of the text, no interpretation, has any more authority than any other. Thus interpretation is as “indeterminate” as the text itself.

On the face of it, such a doctrine would seem to be eminently unhistorical. Historians have always quarreled about the meaning and interpretation of facts and, indeed, about the facts themselves. But they have rarely disputed the reality of the historical past. Philosophers of history have sometimes done this: R. G. Collingwood and Michael Oakshott have come perilously close to it. But historians, working historians, have traditionally assumed some correspondence between interpretation and fact, between language and reality. They are painfully aware of the imperfection of that correspondence—a past that always eludes them, a reality that is never fully revealed to them, a gap between the past as contemporaries experienced and understood it and as the historian tries to recreate and represent it. But they have also been acutely aware of the need to

¹⁸ *Times Literary Supplement* (July 1–7, 1988): 736. See also Bonnie S. Anderson and Judith P. Zinsser, *A History of Their Own: Women from Prehistory to the Present* (New York 1988), xv. Joan Wallach Scott also denies the “parity” of the terms in the “class, race, gender” trinity, suggesting that race and gender are more primary than class (*Gender and the Politics of History*, 30.)

try to close that gap as much as possible, to recapture as much of the past as possible, and to reproduce it as faithfully as possible.¹⁹ These “possibles” have given historians a good deal of latitude, but they have also constricted them a good deal. Today, more and more historians (and not only deconstructionists) are feeling liberated from those constraints and are making the past far more indeterminate, more elusive, less real than it has ever been—thus permitting themselves to be as creative, innovative, and inventive as possible in interpreting the past. These have become the new “possibles” of history: the possibilities suggested by the historian’s imagination and sensibility rather than by the contemporary experience.

In the deconstructionist lexicon (and one hears this now in historical discourse that is not consciously deconstructionist), the “texts” of history are not only the documents of the past, they are the events and facts that are presumed to be as indeterminate as the documents themselves. One historian, celebrating the “process of imaginative creation” that is essential to the “historical imagination,” warns young historians not to become “fact fetishists” like some of their elders.²⁰ It is in this spirit that “facts” appears in quotation marks, and “facticity,” once a pretentious variation on “factuality,” becomes a pejorative word, signifying an excessive concern with facts.²¹ There is no fixed reality in the past, we are told; the whole of the past is indeterminate, including what would seem to be the most obvious matter of fact. “The simplest fact—a dated event—relies on what is for some historians a belief and for others a convenient fiction: the decisive significance of the birth of Christ in establishing a chronology in terms of a ‘before’ and ‘after.’”²² A less sophisticated historian is tempted to point out that the issue is not what some historians happen to believe but what contemporaries believed, and that the “convenient fiction” of a date is a fiction only if it does not correspond to the contemporary experience.²³ If a historian—a Jewish historian, let us say—reporting upon the meeting of the American Historical Association where this paper was delivered, were to date it as 5749, we would surely regard that as evidence of a grave deficiency of historical sense, to say nothing of common sense.

Although deconstructionism, as a conscious, systematic philosophy, has been most prominent among intellectual historians, the mode of thought it repre-

¹⁹ The traditional view was well put by Eleanor Searle in her presidential address to the Medieval Academy of America in 1986 (“Possible History,” *Speculum*, 61 [1986]). To the deconstructionist, this is the fallacy of “realistic objectivism” or “essentialism”—reminiscent of Lenin’s attack on “empirio-criticism” or “naïve empiricism.”

²⁰ Thomas Bender, “‘Facts’ and History,” *Radical History Review*, no. 32 (March 1985): 82–83.

²¹ Carl Schorske, quoted by Peter Novick, *That Noble Dream: The “Objectivity Question” and the American Historical Profession* (Cambridge, 1988), 620.

²² Dominick LaCapra, “Rethinking Intellectual History and Reading Texts,” in Dominick LaCapra and Steven L. Kaplan, eds., *Modern European Intellectual History: Reappraisals and New Perspectives* (Ithaca, N.Y., 1982), 78.

²³ That chronology can be a pitfall has long been recognized by historians. The classic example is George Burr’s exposure, in 1901, of the myth about the end-of-the-world hysteria that supposedly gripped humanity as the year 1000 approached. The moral of this story, as Jacques Barzun and Henry Graff noted, is that historians should have a thorough knowledge of “contemporaneous evidence”—not that they should take a thoroughly relativistic view of all dates and facts. (Jacques Barzun and Henry F. Graff, *The Modern Researcher* [New York, 1957], 104–06.)

sents, even its distinctive vocabulary, is permeating all aspects of the new history.²⁴ Historians now freely use such words as “invent,” “imagine,” “create” (not “re-create”), and “construct” (not “reconstruct”) to describe the process of historical interpretation, and then proceed to support some novel interpretation by a series of “possibles,” “might have beens,” and “could have beens.”²⁵ Another fashionable word is “decode.” One hears of the need to “decode meaning” in terms of gender so as to elicit the power relationships inherent in social relationships;²⁶ or “decoding the past” in terms of psychohistory so as to reveal the psychic reality behind events;²⁷ or “recoding” the entire discipline of history so as to eliminate the “reactionary” effect of narrative history;²⁸ or even decoding the decoders so as to demystify, for example, Michel Foucault himself.²⁹ So, too, we are enjoined to “deprivilege” varieties of “elitist” history—political, diplomatic, intellectual—much as the new literary theorists have “deprivileged” the literary canon. Like those critics who find comic books as worthy of study and respect as the novels of Dickens, distinguished social historians declare the “history of menarche” to be “equal in importance to the history of monarchy,” or Mickey Mouse “more important to an understanding of the 1930s than Franklin Roosevelt.”³⁰

What is being deprivileged and deconstructed is not only history as traditional historians have understood it but the past as contemporaries knew it. Contemporaries may have thought that their history was shaped by kings and statesmen, politics and diplomacy, constitutions and laws. New historians know better. They know that “high politics” are ephemeral and epiphenomenal, to say nothing of being elitist and sexist. They know that workers who accept the legitimacy of the political order or of the “hegemonic culture” are guilty of “false consciousness”

²⁴ Joan Scott finds it especially appropriate for feminist history: “A more radical feminist politics (and a more radical feminist history) seems to me to require a more radical epistemology. Precisely because it addresses questions of epistemology, relativizes the status of all knowledge, links knowledge and power, and theorizes these in terms of the operations of difference, I think post-structuralism (or at least some of the approaches generally associated with Michel Foucault and Jacques Derrida) can offer feminism a powerful analytic perspective” (Scott, *Gender*, 4).

²⁵ See, for example, the exchange between Robert Finlay and Natalie Zemon Davis on *The Return of Martin Guerre*, *AHR*, 93 (June 1988); Hayden White, Review in *Times Literary Supplement* (January 31, 1986): 109–10.

This usage of “invent” is different from another that has become familiar recently. When Eric Hobsbawm and others, in the book of that title, speak of “the invention of tradition” (ed. by Hobsbawm and Terence Ranger, Cambridge, 1983), or when Edmund S. Morgan entitles his latest book, *Inventing the People* (New York, 1988), they are referring to a process of “invention” that took place in the past by contemporaries who were initiating a new concept or practice. In the deconstructionist sense of that word, it is not contemporaries but historians who are doing the “inventing.”

²⁶ Scott, *Gender*, 45.

²⁷ Peter Loewenberg, *Decoding the Past: The Psychohistorical Approach* (New York, 1983).

²⁸ Saude Cohen, *Historical Culture: On the Recoding of an Academic Discipline* (Berkeley, Calif., 1986). See especially chap. 3, “Narrative Summary as a Reactionary Form.”

²⁹ Hayden V. White, “Foucault Decoded: Notes from Underground,” *History and Theory*, 12 (1973).

³⁰ Peter N. Stearns, “Coming of Age,” *Journal of Social History*, 10 (Winter 1976): 250; Warren I. Susman, *Culture as History: The Transformation of American Society in the Twentieth Century* (New York, 1985), 103, 197.

(or “divided consciousness,” or “contradictory consciousness,” or a “half-conscious complicity in their own victimization,” as recent variants have it).³¹

The new history prides itself on rescuing the downtrodden and illiterate from the “enormous condescension of history.”³² In this respect, it has provided a valuable corrective and supplement to the old history. But it is in danger of fostering a new kind of condescension, a condescension toward those contemporaries who left few records of their “consciousness” and are at the mercy of the historian who can “invent,” “imagine,” “create,” or “construct” a consciousness that is suspiciously in accord with the historian’s own consciousness. If it is condescending or demeaning to contemporaries to be ignored, it is surely as condescending and demeaning to make them bear witness not to their own experiences but to those of the historian.

It is one thing, for example, to write women’s history, another to write feminist history. Virginia Woolf appreciated the difference when she urged historians to write about women: “At what age did she marry; how many children had she . . . ; had she a room to herself . . . ?” What is rarely quoted is the rest of that passage, in which she warned them against writing that history from a “conscious bias”: “It is fatal for anyone who writes to think of their sex . . . It is fatal for a woman to lay the least stress on any grievance; to plead even with justice any cause; in any way to speak consciously as a woman.”³³ It is just this conscious bias that is now proudly avowed by historians who announce that they are writing from a “consciously feminist stance,” a “feminist perspective”—a stance and perspective that are those of the historian, not of the subjects of history.³⁴

Race/gender/class—word-processors all over the country must be programmed to print that formula with a single touch of a key. Any part of that trinity involves a considerable revision of the past, but the whole requires nothing less than its deconstruction. The assumption that race, gender, and class are, and always have been, the basic determinants of history deconstructs the past not only as historians have known it but, in many cases, as contemporaries knew it. This is why the concept of “indeterminacy” is so useful. It is only by making the past indeterminate, making it a *tabula rasa*, that historians can impose upon the past their own determinacy. The historian who accepts that trinity must deprive the “authorial voices” of contemporaries who did not do so, who, indeed, did not think in those categories. Those benighted contemporaries, the argument goes, speak with no authority, because they were deluded by the “hegemonic culture” that was itself irredeemably sexist, racist, and elitist. Thus all the past has to be deconstructed and constructed anew.

For some feminist historians, gender alone has that effect. “To place women at the center and make sense of their experiences meant reconceptualizing

³¹ Symposium by Jackson Lears, John P. Diggins, George Lipsitz, *et al.*, in the *Journal of American History*, 75 (June 1988): 137–61.

³² E. P. Thompson, *The Making of the English Working Class* (New York, 1964), 12.

³³ Virginia Woolf, *A Room of One’s Own* (1929; rpt. edn., New York, 1945), 39, 86.

³⁴ Joan B. Landes, *Women and the Public Sphere in the Age of the French Revolution* (Ithaca, N.Y., 1988), 1–2; Scott, *Gender*, 3, 6.

European history so that we could understand what history would be like 'if it were seen through the eyes of women and ordered by values they define.'"³⁵ Another explains that "the writing of women into history" requires so complete a redefining of historical experience and significance that it implies "not only a new history of women, but also a new history."³⁶ This is truly "total history"—far more total than anything intended by the social historians who originally used that phrase. It is a total indictment of all of history, in both senses of that word—the past itself and all of previous writing about the past—and an invitation to the total rewriting of history, again in both senses of that word.

All of history, we are told, is political—not in the naïve sense of the Victorian historian who pronounced history to be "past politics" but in the modern sophisticated sense in which history is regarded as "present politics." And politics is expanded to mean, à la Foucault, power relations in the broadest sense—sexual, personal, racial, social. Thus the past itself, as well as historical writing about the past—and, indeed, any pretense of knowledge or truth about either the past or the present—is seen as a reflection of power relations.³⁷ It is a proposition of breathtaking grandeur and simplicity, so grand and so simple that it defies all empirical proof or disproof.³⁸ It also reduces all historical controversy to *ad hominem* arguments, since all historians are presumed to be expressing or defending their own positions of power. To this mode of argument, one can respond with a *tu quoque* retort—or better yet, one may choose not to demean oneself and historical discourse by responding at all.

This view of history recalls John Stuart Mill's account of the essential difference between the two seminal thinkers, Jeremy Bentham and Samuel Coleridge. Both, he said, could rightly claim to be "great questioners of things established." But each asked a different question. Bentham asked, "Is it true?" while Coleridge asked, "What is the meaning of it?"

The one [Bentham] took his stand *outside* the received opinion, and surveyed it as an entire stranger to it; the other [Coleridge] looked at it from within, and endeavoured to see it with the eyes of a believer in it; to discover by what apparent facts it was at first suggested, and by what appearances it has ever since been rendered continually credible—has seemed to a succession of persons, to be a faithful interpretation of their experiences.³⁹

I take this to be the essential distinction between the new history and the old. The new history stands outside the received opinion—the opinion of contemporaries as well as traditional historians—and is prepared to pronounce it simply false. The old history stands within the received opinion, trying to understand it

³⁵ Anderson and Zinsser, xviii, quoting Gerda Lerner, *The Majority Finds Its Past: Placing Women in History* (New York, 1979), 168.

³⁶ Scott, *Gender*, 29, quoting Berenice Carroll, ed., *Liberating Women's History* (Urbana, Ill., 1976), 89; Scott, 27, on the "rewriting" of history.

³⁷ Scott, *Gender*, 2, 26–27. See Allan Megill, "The Reception of Foucault by Historians," *Journal of the History of Ideas*, 48 (1987).

³⁸ Foucault is one of the subjects discussed in the aptly named volume, *The Return of Grand Theory in the Human Sciences*, ed. Quentin Skinner (Cambridge, 1985).

³⁹ John Stuart Mill, "Coleridge," in *Essays on Politics and Culture*, ed. Gertrude Himmelfarb (New York, 1962), 133.

as contemporaries did, to find out why they believed what they did, why those beliefs seemed to them “credible,” a “faithful interpretation of their experiences.” The new history has much to teach the old. But this is one lesson the old may teach the new.

AHR Forum
The Unpredictable Past:
Reflections on Recent American Historiography

LAWRENCE W. LEVINE

"THE FUTURE IS CERTAIN," a Soviet joke assures us, "it is only the past that is unpredictable."¹ For historians, alas, the future is hardly certain, and the fact that the past is unpredictable has become no laughing matter. Indeed, it has caused a good deal of concern in recent years and the growing conviction among many that the venerable craft of the historian is in a state of crisis.

This was not always so. The unpredictability of the past was an accepted fact to those who thought seriously about history. The dialectic between the past and the present was seen not only as inescapable but as salutary. History, Henry James observed, "is never, in any rich sense, the immediate crudity of what 'happens,' but the finer complexity of what we read into it and think of in connection with it."² This truth was built into the graduate program at Columbia University when I was a student there thirty years ago. On our comprehensive written examinations, there was one question that was repeated year after year: "Discuss the changing interpretations of: . . ." which was followed by a varying list of the basic events: the American Revolution, the creation of the Constitution, Jeffersonian Democracy, the causes of the Civil War, the nature of the Populist revolt, and on and on.

Historians were, and I think still are, comfortable with the fact that our understanding of these standard historical occurrences inevitably varies from generation to generation because they perceive that, of necessity, we view them through the prism of a changing present. Nor have historians conceived of this prism as a prison that condemned them to a flawed vision. There was confidence that, although we might operate in cycles of historical interpretations, the general movement was upward toward an increasingly sophisticated understanding of the past. The present was not merely a hindrance; it could provide new ways of seeing things, new tools, new perceptions of human motivation or economic forces that helped us to gain a surer sense of past generations. When the French historian Marc Bloch made the familiar observation, "Misunderstanding of the present is the inevitable consequence of ignorance of the past,"

¹ Quoted in Stephen Lukes, *Marxism and Morality* (Oxford, 1985), 146.

² Henry James, *The American Scene* (1907; rpt. edn., New York, 1946), 182.

he quickly added, "But a man may wear himself out just as fruitlessly in seeking to understand the past, if he is totally ignorant of the present."³

There is one area of historiographical unpredictability, however, with which many historians have not learned to make their peace. This involves not changing interpretations of well-agreed-upon standard events but changing notions of which events—and which people—should constitute the focus of the historian's study.

The direction in which American historiography has moved during the past several decades becomes clear by discussing the ideas of a prominent and influential American historian, John Higham of Johns Hopkins University. In two articles, "The Cult of the 'American Consensus'" and "Beyond Consensus," that he published in 1959 and 1962 respectively, Higham took his colleagues to task for treating our history much the way dairy companies treated our milk: homogenizing it so that all the separate particles were blended together in one indistinguishable mass. Earlier historians, Higham insisted, "had painted America in the bold hues of conflict": class against class, section against section, ideology against ideology, and had viewed the American past as jagged and discontinuous, filled with cataclysms and sudden change. For historians in the conservative years after World War II, on the other hand, American history looked more like one long, happy voyage with few fundamental differences and with an enduring consensus. "The phrase '*the American experience*' has become an incantation," Higham charged, as he pointed to his colleagues' emphasis on uniformity, stability, "the persistence of a national character, the triumph of a single homogeneous culture."⁴

More than twenty years later, at the 1983 annual meeting of the Organization of American Historians, Higham issued another critique of the direction in which his colleagues were moving. In a paper titled "Beyond Pluralism: the Historian as American Prophet," Higham argued that somewhere in the late 1960s the ruling paradigms of homogeneity and consensus were replaced by the paradigms of fragmentation and heterogeneity. The ideal of the national community was replaced by the ideal of the local community: the town, the parish, the family, the ethnic group. Thus the very things Higham had been decrying in the late 1950s and early 1960s—the exaggerated belief in consensus and homogeneity—had been vanquished. But Higham was far from pleased. The more we learned about the specific, he complained, the less we understood about the larger scheme of things. Historians had lost their sense of coherence and direction. Higham described his colleagues by using the metaphor of a field of solitary gophers, each digging its own hole. Our task now, Higham concluded, was to rediscover our national unity and our national identity.⁵

What had happened between the early 1960s and the early 1980s to lead

³ Marc Bloch, *The Historian's Craft* (New York, 1962), 43.

⁴ John Higham, "The Cult of the 'American Consensus': Homogenizing Our History," *Commentary* (February 1959): 93–100; "Beyond Consensus: The Historian as Moral Critic," *AHR*, 67 (April 1962): 609–25.

⁵ John Higham, "Beyond Pluralism: The Historian as American Prophet," unpublished paper delivered to the Organization of American Historians, April 1983.

Higham to change the title of his critique from "Beyond Consensus" to "Beyond Pluralism," and to lament at the end of this period the demise of the very thing he seemed to be inveighing against at its beginning? For our purposes, the most decisive factor was the advent of a stream of modern American historiography beginning in the late 1960s and blossoming in the mid-1970s that was—and is—convinced that there can be no real sense of the whole without exploring the parts, without understanding—often for the first time—the consciousness and actions of workers, women, ethnic, religious, racial, and national minorities, immigrants and their progeny, who participated in a myriad of separate geographical, occupational, fraternal, and religious communities that together constituted the larger society.

This shift was never total or inclusive. Throughout the years since the mid-1970s, historians continued to explore all the standard phenomena that have traditionally concerned them. Revisionism characteristically and perhaps inevitably tends to overstate. But these overstatements—especially the occasionally stupid ones—should not be elevated into an expression of what the new approaches are all about. The great majority of those who have sought to expand our historical vision to new groups of people and new areas of expressive culture mean to do just that: to *expand* our knowledge, to *supplement* our approaches, not to erect new fences and shut still more doors.

In 1965, I published a study of the politician William Jennings Bryan that tried to use him to understand his small-town and agrarian midwestern and southern constituencies. In the years following the publication of my Bryan study, I turned to a history of black protest thought in America and attempted to utilize the same approach: that is, I began by focusing on the thought of the black leadership. Increasingly, I was troubled by my assumption that one necessarily can derive the consciousness of people from the goals and aspirations of their leaders, and I turned gradually and then decisively to folk materials to enable me to hear more directly the voices of people who had not left traditional historical sources behind them. Although I felt quite lonely and vulnerable as I was writing this work, this, as it turned out, was precisely the direction in which many historians, especially younger ones, were moving.⁶

The new historiography was fashioned by what has been perhaps the most important intellectual breakthrough by historians in the past two decades: their changed attitude toward the folk whom they now began to see not as inarticulate, impotent, irrelevant historical ciphers continually processed by forces over which they had no control but rather as actors in their own right who, to a larger extent than we previously imagined, were able to build a culture, create alternatives, affect the situation they found themselves in, and influence the people they found themselves among.

This historiographical trend has brought forth a series of laments from both within and without the profession. Once, we have been told, American history

⁶ Lawrence W. Levine, *Defender of the Faith: William Jennings Bryan, The Last Decade, 1915–1925* (New York, 1965); *Black Culture and Black Consciousness: Afro-American Folk Thought from Slavery to Freedom* (New York, 1977).

was written by those who had a sense of the entire tradition, who wrote of a nation, not of its bits and pieces, whose writings did not divide the American people into their constituent elements but helped unite them by giving them a sense of a common tradition and a shared past. In 1982, C. Vann Woodward deplored the “fragmentation of the profession into highly specialized fields.” “People of a democratic tradition,” he insisted, “can surely be interested in the historic plight of the powerless, but they have a natural and abiding concern for power and those who have wielded it and to what effect—a concern that historians should never have neglected anyway. If they can now revive the art of the craft, historians can also reclaim a general public.”⁷

This tendency to decry the historiographical present and look with nostalgia upon the historians of the nineteenth and early twentieth centuries, whose sweeping, politically focused narratives supposedly encompassed the whole tradition and communicated with a wide audience, smacks of what Eric Hobsbawm has called “invented tradition.”⁸ In reality, for the most part, these earlier historians were concerned overwhelmingly with a decided minority of the population in terms of class, ethnicity, region, and gender, and tended to confuse the history of one group with the history of the nation. Moreover, historians today probably communicate with a much broader public than their predecessors ever did, both through the secondary schools and colleges, which contain a far larger portion of the population than at any other time, and through such institutions as the National Museum of American History, whose recent popular exhibits on the black migration to Chicago and the internment of Japanese-Americans after Pearl Harbor could not have taken place without the new scholarship, whose ultimate influence cannot be measured by the direct sales of books, which is never the way to gauge the influence of scholarship either in the sciences or the humanities.

But, if the practice of creating a Golden Age of Historiography, in comparison with which current historians pale, cannot be taken literally, it helps us to understand the nature of the debate we are engaged in. This practice was manifest at least as early as 1962, when the current historiographical stream was still a trickle. In his presidential address to the American Historical Association that year, Carl Bridenbaugh of Brown University painted a bleak picture of the future of the historical profession in the United States. In former days, Bridenbaugh maintained, the ablest historians were amateur scholars, men who had known life at first hand, and shared a common culture. “Today,” he lamented,

we must face the discouraging prospect that we all, teachers and pupils alike, have lost much of what this earlier generation possessed, the priceless asset of a shared culture. Today imaginations have become starved or stunted . . . Furthermore, many of the younger practitioners of our craft, and those who are still apprentices, are products of lower middle-class or foreign origins, and their emotions not infrequently get in the way

⁷ C. Vann Woodward, “A Short History of American History,” *New York Times Book Review*, August 8, 1982.

⁸ Eric Hobsbawm and Terrence Ranger, eds., *The Invention of Tradition* (Cambridge, 1983), 1–14.

of historical reconstructions. They find themselves in a very real sense outsiders on our past and feel themselves shut out. This is certainly not their fault, but it is true. They have no experience to assist them, and the chasm between them and the Remote Past widens every hour . . . What I fear is that the changes observant in the background and training of the present generation will make it impossible for them to communicate and to reconstruct the past for future generations.⁹

Bridenbaugh's jeremiad was met with embarrassed silence and treated, when it was treated at all, as some sort of idiosyncratic aberration. While it is true that no other scholars have picked up his unfortunate tendency to read an entire group of young historians out of the profession by asserting that the culture they were raised in forever barred them from comprehending the American past, it is also true that Bridenbaugh was quite prescient in one important respect: the young historians then preparing to take their place in the profession *were* different from the generations before them in their greater degree of heterogeneity and marginality and these differences *did* leave their mark on the generation's historical work.

Where he went wrong was in his prediction that that mark would have primarily negative results. In fact, it is no accident that the heterogeneous historical generation Bridenbaugh so feared turned out to understand one fundamental aspect of the American past—its heterogeneity—infinately better than its predecessors had. The decades following Bridenbaugh's presidential address were characterized historiographically by an exciting and pioneering dynamic in which historians explored uncharted groups and institutions; made the expressive culture of the folk and of popular entertainment part of American culture; wondered openly about the direction of cultural diffusion and hypothesized that cultural influence could proceed from the socioeconomic bottom to the top as well as vice-versa. The generation that Bridenbaugh predicted would not and could not understand the American past has done more to enable us to understand it in its full complexity and diversity than any preceding generation of historians.

It is this very accomplishment that has proven to be most troubling to so many. Although the current debate is usually billed as political versus social and cultural history, or narrative history versus analytic history, or fragmentary history versus synthetic history, it makes complete sense only when it is seen as what, at its root, it really is: a debate about the extent to which we should widen our historical net to include the powerless as well as the powerful, the followers as well as the leaders, the margins as well as the center, popular and folk culture as well as high culture. The primary criticism of contemporary historiography has little to do with what kind of history we practice and almost everything to do with the subjects of that history. This is really what is objected to by those who so fear the directions in which many contemporary historians are moving. When Gertrude Himmelfarb, who has written of what she terms "the current prejudice against greatness," charges, on page 17 of her book, *The New History and the Old*, that for the social historian "the infrastructure is what the historian thinks it is,

⁹ Carl Bridenbaugh, "The Great Mutation," *AHR*, 68 (January 1963): 322–23.

not what contemporaries may have judged to be the most significant aspects of their lives and times,” we arrive at the nub of the problem: how broad, how comprehensive must our history be to know what “contemporaries” thought was significant? How far must we go beyond the political, economic, and military leaders even to begin our understanding of political, economic, and military history? Himmelfarb tells us on the very next page by the insertion of two revealing adjectives before the word “contemporaries.” Social history, she writes, by devaluing the political realm “makes meaningless those aspects of the past which *serious* and *influential* contemporaries thought most meaningful” (italics added). The point is made even more clearly eight pages later when Himmelfarb complains that “the social historian, rejecting any such ‘elitist’ idea as the good life, seeking only to understand *any* life, indeed regarding it as a triumph of the historical imagination to explore the lowest depths of life, to probe the unconscious, unreflective, irrational aspects of life, denies that man is the distinctive, indeed unique animal Aristotle thought him to be—a rational animal, which is to say, a political animal.” Indeed, Himmelfarb goes so far as to assert that the new history “involves a radical redefinition of human nature.”¹⁰

To comprehend why such apocalyptic language is used—and it is used often, as witness Theodore Hamerow’s charge that many of the new historians feel “they must be ruthless in destroying conventional methodology”—it is necessary to locate the historiographical debate within the larger debate concerning the current directions of American culture itself: a debate in which William Bennett, while he was still Secretary of Education, accused those faculty desiring to expand the cultural canon of “trashing” Plato and Shakespeare.¹¹ The larger contemporary debate between those like Allan Bloom on the one side who “know” what culture is and what it is not, who have a map of its fixed perimeters and a profile of the identity of its creators and its followers, who perceive culture to be something finite and fragile, which needs to be conserved and protected from the incessant Philistinism that threatens it, and those on the other side who believe that worthy, enduring culture is not the possession of any single group or genre or period, who conceive of culture as neither finite nor fixed but dynamic and expansive, helps to frame our own specific historical debate and makes it clear that our discussions are not taking place in some academic vacuum apart from the outside world.¹²

But there is still another context that helps to elucidate the full meaning of our debate. The complaints against the current historiography center not just on its subject matter but on its degree of complexity and relative inaccessibility to a larger public. We first heard charges like these not in the discipline of history but in that of physics and the sciences in general. During the 1920s, the *New York Times* published a series of editorials bitterly lamenting the fact that educated people could no longer grasp those theories on the cutting edge of science.

¹⁰ Gertrude Himmelfarb, *The New History and the Old* (Cambridge, 1987), 17–18, 25–26, 56.

¹¹ Hamerow’s charge is in Theodore S. Hamerow, *Reflections on History and Historians* (Madison, Wis., 1987), 169. Bennett was quoted in the *San Francisco Chronicle*, February 5, 1988.

¹² Allan Bloom, *The Closing of the American Mind* (New York, 1987); Lawrence W. Levine, *Highbrow/Lowbrow: The Emergence of Cultural Hierarchy in America* (Cambridge, 1988).

"What common folk must be content to do in regard to Dr. Einstein," the *Times* concluded reluctantly in 1921, "is to accept the judgment of experts on his work, just as they do in many other domains of specialized knowledge." The *Times* seemed loath to accept its own advice, however, and continued to return to the problem throughout the decade, always with the same negative results. In a 1923 editorial titled "IT SIMPLY CAN'T BE DONE" the *Times* asserted, "The attempt to explain his theory to people who know only the elements of arithmetic, algebra and geometry is quite hopeless. The thing cannot be done, and, however humiliating the confession may be, most of us will be obliged either to take our Einsteinism on trust or just ignore it." Five years later, the paper used an editorial titled "A MYSTIC UNIVERSE" to proclaim, "Countless textbooks on Relativity have made a brave try at explaining and have succeeded at most in conveying a vague sense of analogy or metaphor . . . Understanding the new physics is like the new physical universe itself . . . We can only hope for dim enlightenment. The situation is all the harder on the public because physics has become unintelligible precisely in an age when the citizen is supposed to be under the moral obligation to try to understand everything. Nor are things made easier for the common man . . . when theory changes from year to year."¹³

In a recent survey of modern physics, Nick Herbert wrote of "the reality crisis in physics." Quantum theory, he observed, "irreparably smashed Newton's clockwork" and taught us that the world is *not* a deterministic mechanism, but did not teach us what the world *is*.¹⁴ The late Nobel Laureate Richard Feynman had this advice for those interested in quantum mechanics: "Do not keep saying to yourself, if you can possibly avoid it, 'But how can it be like that?' because you will go 'down the drain' into a blind alley from which nobody has yet escaped. Nobody knows how it can be like that."¹⁵

History, of course, is not physics, and historians neither can nor want to claim the level of complexity and abstraction attained by Albert Einstein and his colleagues. Nonetheless, historians today are engaged in a similar debate centering on matters of synthesis, complexity, and accessibility because their own discipline and related humanistic disciplines do not inhabit a planet apart from the sciences; they are part of the same cultural matrix out of which the new scientific attitudes and approaches have sprung.

In 1928, the *New York Times* observed that, while Alfred Tennyson defined faith as believing what we cannot prove, "The new physics comes perilously close to proving what most of us cannot believe; at least until we have rid ourselves completely of established notions and forms of thought . . . The Quantum invites us to think of something which can be in two places at the same time."¹⁶

¹³ *New York Times*, editorials, July 8, 1921, February 6, 1923, January 28, 1928. See also editorials in the issues of April 26, 1920 and January 30, 1922.

¹⁴ Nick Herbert, *Quantum Reality: Beyond the New Physics* (Garden City, N.Y., 1985), xi-xii, 246, and *passim*.

¹⁵ Quoted in Heinz R. Pagels, *The Cosmic Code: Quantum Physics as the Language of Nature* (New York, 1983), 113.

¹⁶ *New York Times*, January 28, 1928.

Much of contemporary historiography invites us to rid ourselves of established notions and to stretch our imaginations in similar ways.

If the *Times* had trouble with the concept that something can be in two places at the same time, what are we to say when we learn, for instance, that a culture can move in two directions at the same time? In my own work on Afro-American folk thought, I was searching for that moment in post-Civil War history when American blacks, whose culture during slavery had remained so very African in so many ways and so autonomous in so many respects from the white culture that surrounded it, crossed over into the process of unmistakable cultural amalgamation. But I found that, every time I focused on a new form of cultural expression that seemed to function as a mechanism for deep acculturation to the larger society, I discovered important degrees of cultural revitalization as well. In so many modern forms of their expressive culture, Afro-Americans exhibited the tendency to surge outward into the larger society even as they nurtured a strong centripetal urge that continually drew them back to central aspects of their traditions. My own findings were duplicated by the sociologist Mark Slobin in his study of Jewish music in America. We each independently found that such recent developments as the modern recording industry acted not only as a force for cultural amalgamation but also served to preserve important elements of group tradition. That is, developments that common sense and logic tell us *must* have reduced a group's cultural autonomy turn out on closer inspection to have been more complex and to have enabled people to move simultaneously outward and inward. Thus it is difficult to disagree with Slobin's rejection of the older progressive models of cultural change and accommodation in favor of the recognition that what we are faced with is "a dynamic state of flux."¹⁷

Contemporary scholars have demonstrated again and again that, in penetrating the culture of a neglected group, historians often find more than they bargained for. What looked like a group becomes an amalgam of groups; what looked like a culture becomes a series of cultures. Americans on the eve of World War II might have seen only a monolith when they looked at Japanese-Americans, but historians must see something vastly more complicated: the *Issei* born in Japan and legally barred from becoming U.S. citizens, the *Nisei*, born and raised here and thus citizens by birth, the *Kibei*, born here but raised in Japan and thus legally Americans and culturally Japanese, as well as those who lived in cities and those who lived on farms, those who struggled to maintain the old ways and those who hungered for acculturation. The complexity I speak of is not the complexity of specialized languages or esoteric methodologies but the complexity of people and the cultures they create.

This is not to say that we must fragment every group we study to the point where generalizations become impossible, but if we generalize the things we study right out of their complexity, we are doomed to futility. There can be no meaningful political history of labor or industry in the United States without understanding the cultures and social structures of the urban-industrial working

¹⁷ Lawrence W. Levine, *Black Culture and Black Consciousness* (New York, 1977); Mark Slobin, *Tenement Songs: The Popular Music of the Jewish Immigrants* (Urbana, Ill., 1982).

class—the bulk of them immigrant and black—at the turn of the century. There can be no complete political history of women's rights in the United States without an understanding of the impact of the new immigrants, so many of whom came from Southern and Eastern European cultures in which women occupied a decidedly subordinate place. We have not begun to understand our political history sufficiently because we have too frequently artificially separated it from the larger cultural context of which it was a part.

Just as scientists have traveled light years from Copernicus's conviction that the Ptolemaic universe must be a misconception because God would not have expressed Himself in such complicated terms, and have spent much of their time attempting to articulate the complexities of the universe we do inhabit, so, too, historians find themselves again and again complicating simple pictures, finding intricacies where before we had certainties, turning unity into multiplicity, clarity into ambiguity. It is possible to see this development, as a number of critics have, as an act of perversity and failure. It is also possible to view it, as I do, as a sign of the maturation and seriousness of our profession. In any case, it is in the midst of this fluid state that we are hearing increasingly insistent demands that we get our act together, carry the enterprise forward, formulate syntheses that allow us to make our current findings immediately intelligible to a broad public, and incorporate larger doses of narrative style in our work, as our illustrious scholarly ancestors did.

We have to comprehend, finally, that we ourselves have a good deal to do with the reception of the history we write and teach. If we tell people continually that history is invariably narrative storytelling about those whose power, position, and influence are palpable, then that is precisely what they will expect from us. But this is only one form of history, and it is incumbent upon us to inform the public, by deed and word, that there is no exclusive preferred form for the writing of history and that no single group in history and no one aspect of the past—the social, the political, the cultural, the economic—is inherently more important, or more essential, or more relevant than the others. If we have respect for our audience, then we must realize that ambiguity and paradox and uncertainty are not strangers to them. They know these things are part of life, and they certainly can be taught to see them as part of history.

But historians will be in no position to teach these lessons if we ourselves do not strive to increase not only our tolerance for and acceptance of the complexities of the past but our tolerance for and acceptance of the complexities and ambiguities of our own profession.

AHR Forum
History in Crisis?
The Others' Side of the Story

JOAN WALLACH SCOTT

The cry goes up that one is murdering history whenever, in a historical analysis . . . one is seen to be using in too obvious a way the categories of discontinuity and difference, the notions of threshold, rupture and transformation, the description of series and limits. One will be denounced for attacking the inalienable rights of history and the very foundations of any possible historicity. But one must not be deceived: what is being bewailed with such vehemence is not the disappearance of history, but the eclipse of that form of history that was secretly, but entirely related to the synthetic activity of the subject; . . . what is being bewailed, is that ideological use of history by which one tries to restore to man everything that has unceasingly eluded him for over a hundred years.

Michel Foucault, *The Archaeology of Knowledge*, 14.

"HISTORY IS PAST POLITICS and politics present history" was the founding motto of the American Historical Association. As he busily organized our profession, Herbert Baxter Adams regularly cited the English historian Edward Freeman's formulation. (Indeed, he cited it so often that some people began to think that Adams had written it himself.) At a time when social history has broadened the discipline's focus, it may seem anachronistic, if not foolish, to reinvolve the old adage. But that is what I want to do. I think I am on safe ground because my definitions of "politics" and "history" have been updated to take account of contemporary usage. When that is done, the old words have new and relevant resonance.

By "politics," I mean not only formal operations of government but contests that involve power in Michel Foucault's sense—power not only as a relationship of repression or domination but also as a set of relationships or processes that produce positive effects: social consensus about the meanings of truth, the

I am grateful to Donald Scott for his comments and close critical readings of many drafts of this paper and to Susan Harding, Chantal Mouffe, and Elizabeth Weed for their useful and important suggestions. The criticisms of Clifford Geertz and Carl Schorske led me to make important revisions in the structure of the argument. As a result, this is a somewhat different paper from the one I read at the American Historical Association meetings in December 1988.

hegemony of certain systems of knowledge (science in the nineteenth century), the disciplinary regimes of academic fields such as history.¹ By “history,” I mean not what happened, not what “truth” there is “out there” to be discovered and transmitted, but what we *know* about the past, what the rules and conventions are that govern the production and acceptance of the knowledge we designate as history. My first premise is that history is not purely referential but is rather constructed by historians. Written history both reflects and creates relations of power. Its standards of inclusion and exclusion, measures of importance, and rules of evaluation are not objective criteria but politically produced conventions. What we know as history is, then, the fruit of past politics; today’s contests are about how history will be constituted for the present. “History is past politics and politics present history.”

My rereading of Freeman accepts his equation of politics and history but in somewhat different terms. While he meant, I think, that history was always *about* politics, I mean that history is inherently political. There is no single standard by which we can identify “true” historical knowledge, however well-trained we have been in graduate seminars on methods and historiography. Rather, there are contests, more and less conflictual, more and less explicit, about the substance, uses, and meanings of the knowledge that we call history.

I am not here referring to topical debates or to differences of interpretation about, say, the origins of the French Revolution or the causes of the American Civil War. Rather, I am talking about the process by which a particular approach to historical inquiry claims to embody the entire discipline by defining itself as “History” and declaring challenges to it to be non-history, unacceptable and irrelevant because outside the boundaries of the field. This process is about the establishment and protection of hegemonic definitions of history.

One can follow the process in the records of the American Historical Association since its founding in 1884, especially in presidential addresses, those ritual occasions that encourage disquisitions on the state of the field.² Books and articles about the condition of the profession are also often exercises in boundary maintenance, or, less frequently, challenges to prevailing rules. In them, we find evidence of debate and disagreement, moments of dramatic challenge to orthodox views about the subject matter and philosophy of history, and, above all, repeated attempts by guardians of orthodoxy to maintain unquestioned predominance for their point of view by insisting that only they represent “truth,” or “science,” or “objectivity,” or “tradition,” or “history-as-it-has-always-been-written.”³

The rhetoric of these texts is surely inflated, and their extreme normative

¹ Michel Foucault, “Truth and Power,” in Colin Gordon, ed. *Power/Knowledge: Selected Interviews and Other Writings, 1972–1977* (New York, 1980).

² Presidential addresses are sometimes idiosyncratic observations and sometimes reviews of the literature in the president’s speciality, but they are more often statements about what the reigning definition of history is or ought to be.

³ By far the best source for these debates is Peter Novick, *That Noble Dream: The “Objectivity Question” and the American Historical Profession* (Cambridge, 1988). I have relied heavily on Novick’s insights and references throughout this essay.

statements conceal the range and variety of philosophies contained in most substantive history writing. That, of course, is their function and their importance; by polarizing debate, they seek to establish criteria of membership in the profession—to include some and exclude others. And they seek to enlist the support among historians and a broader public that will guarantee that their definition of history prevails.

Since this seems to be one of the moments of great contest about the meaning of history and since political conservatives have raised their banner in defense of a very particular monolithic, unitary, and universal view that they equate with “History,” it seems worthwhile to analyze their rhetoric and the historical validity of their claims. Through this analysis, I hope not only to expose the highly political nature of the conservatives’ defense of their version of “History” but also to show that it has had profoundly exclusionary and elitist effects on the discipline.

For those who think their position is or ought to be hegemonic, the appearance of critical challenges constitutes a “crisis.” By representing themselves simply as the guardians of “History,” they deny the possibility of fundamental disagreement about the boundaries of the field, instead representing those who challenge these boundaries as outsiders to history, as either ignorant or willfully destructive enemies. We have a recent example in Alan Kors’s hysterical claim (as reported in the *New York Times* in November 1988) that “the barbarians are in our midst.” “We need to fight them a good long time,” he said, in a display of masculine bravery. “Show them you are not afraid, they crumble.”⁴ These “barbarians” turn out to be those who advocate increased representation of women and minorities on faculties and in the curriculum.⁵ Their demands might constitute, for some, evidence of the wonders of democratic pluralism, but, for Kors and his colleagues, they are nothing less than a subversion of our national heritage and of the entire legacy of Western civilization.

In the “history” defended by the self-designated guardians of orthodoxy, boundaries are said to be permanent and rules unquestionable; challenges are not deemed legitimate, rather they are denied or repressed. A chaotic present and a disastrous future are counterposed to a nostalgic vision of an intact and uncontested intellectual world that we are losing or have already lost. This nostalgic vision presents the so-called orthodox view of history as the only “History” that has existed until the current “crisis.” It is a vision secured, moreover, not through concrete illustration or close examination of evidence but through negative contrast. In recent debates on curricular change, we have

⁴ Joseph Berger, “Scholars Attack Campus ‘Radicals,’” *New York Times* (November 16, 1988): A22.

⁵ My source is, as cited, the *New York Times* report on the conference. Alan Kors, in a personal correspondence to me (January 9, 1989), insists that the unedited tapes of the meeting show him to hold a different position from that reported in the *Times*. “The object of my criticism are overwhelmingly white and male,” he writes in his letter. He also claims to be against “group-identity,” “above all, in fact, racial and gender group-identity, as the basis for judgment or treatment of people.” I read that as a position to affirmative action, and that is how the *Times* report characterized it. The correction of the November 19 story, apparently elicited by Kors, does not change the point I make about his speech in this essay. See “Editors’ Note,” *New York Times* (February 6, 1989): 3.

heard again and again that the “traditional” curriculum is being discarded for new and faddish courses (on, among other things, the history of the civilizations of India or China). Lynn Cheney, the head of the National Endowment for the Humanities, has written that there are timeless truths “transcending accidents of class, race, and gender, [that] speak to us all.” She implies that everyone always agreed on what these were until very recently, when for the first time knowledge has become “politicized.”⁶ Similarly, Gertrude Himmelfarb’s book title poses the “old” history—the “traditional” way history has presumably always been done—against the subversive and illegitimate “new.”

These arguments insist that there is only one way to conceive of history, only one standpoint for the historian. Over the years, these kinds of monolithic conceptions have stressed continuity, moral values, the importance of the individual, and the example of law, government, and politics as the highest expression of “man’s” reason in restraint of self-interest.⁷ If exclusive emphasis on Christianity (“which we may fairly say is to-day the strongest of any in its influence upon human history,” as Simeon Baldwin put it in his presidential address to the AHA in 1906) has declined, the insistence on timeless moral “values” remains.⁸ Overt racist statements have disappeared—such as John Burgess’s comment in 1902 that “a black skin means membership in a race of men which has never of itself succeeded in subjecting passion to reason.”⁹ But the racism implicit in some of today’s assertions about the “nobility” and “superiority” of Western civilization persists.¹⁰

In the past, historians committed to a monolithic view also tended to focus on elites (those Goldwin Smith in his presidential address of 1904 referred to as “the leading members of the race”¹¹), assuming that their experience embodied “man” at his best and that their power was the deserved result of higher intellect, manners, morals, and aesthetic taste. Until relatively recently, this elite position was taken to be the most compelling subject of history. I use the word subject in its grammatical sense and with double implications: to refer to those actors deemed historical agents and to historians, those who actively produce historical knowledge. At least until after World War II and in some cases until the 1960s, the prevailing description of the ideal historian bore remarkable similarity to the characterization of elites. It was as if only those who were of the elite (or who could manage to acquire their tastes and outlook) could stand high enough to command an unrestricted view of the whole past. All other views were deemed to be partial at best, interested at worst. The possibility of telling a coherent, unitary story seemed to rest on the preservation of the homogeneity of the

⁶ “Text of Cheney’s ‘Report to the President, the Congress, and the American People’ on the Humanities in America,” *Chronicle of Higher Education* (September 21, 1988): A20.

⁷ Gertrude Himmelfarb, *The New History and the Old: Critical Essays and Reappraisals* (Cambridge, Mass., 1987), 20.

⁸ Simeon E. Baldwin, “Religion Still the Key to History,” *AHR*, 12 (January 1907): 243.

⁹ John W. Burgess, *Reconstruction and the Constitution: 1866–1876* (New York, 1902), 133, cited in Novick, *That Noble Dream*, 75.

¹⁰ Robert Nesbit, *History of the Idea of Progress* (New York, 1980), 317, cited in Himmelfarb, *New History and the Old*, 169.

¹¹ Goldwin Smith, “The Treatment of History,” *AHR*, 10 (April 1905): 520.

profession. For some, this was a literal homogeneity of elite membership; for others, it was (and still is) a singular elite outlook that might be held by different kinds of people.

The description of the ideal historian has been offered at least since the 1920s in statements lamenting the degradation of the profession by the entry into it of non-elites. These laments differ in emphasis and detail, but they show a consistent preoccupation with setting the rules by which orthodoxy can be maintained. In 1926, John Spencer Bassett, then-secretary of the AHA, wrote of the loss of the "aesthetic sense" to history that accompanied the arrival of "persons who have sprung from the class that is accustomed to the plainer ways and thinking of the world." "It would be untrue and also unkind to say that these persons do not make good teachers of their subjects . . . some of them show, despite their early lack of taste, remarkable grasp of its quality. But the majority take a long time to acquire it, and some never manage to reach it."¹²

In this, as in later discussions, the upper classes were synonymous with a white, Protestant establishment. Anti-Semitism, as well as anti-Catholicism, reigned in the historical profession. Peter Novick's extensive and close reading of the papers of historians has turned up some telling documentation of the awareness of the prevalence of discrimination against "ethnics" in the 1920s and 1930s. A letter written on behalf of Oscar Handlin, for example, stated that "he has none of the offensive traits which some people associate with his race." Another, supporting Bert Lowenberg's application for a job said, "by temperament and spirit . . . [he] measures up to the whitest Gentiles I know." And yet another avowed that Richard Leopold was "of course a Jew, but since he is a Princeton graduate, you may be reasonably certain he is not of the offensive type."¹³ The point of these letters, of course, was that, despite their origins, the candidates had acquired the requisite perspective for a historian.

The discussion of the deterioration of professional standards caused by the entry of lower-class men continued into the postwar period unabated. George Pierson, chairman of Yale's history department, wrote to President Griswold in 1957:

Apparently the subject of English still draws to a degree from the cultivated, professional, and well-to-do classes . . . By contrast, the subject of history seems to appeal on the whole to a lower social stratum . . . Far too few of our history candidates are sons of professional men; far too many list their parent's occupation as janitor, watchman, salesman, grocer, pocketbook cutter, bookkeeper, railroad clerk, pharmacist, clothing cutter, cable tester, mechanic, general clerk, butter-and-egg jobber, and the like. One may be glad to see the sons of the lower occupations working upward . . . It may be flattering to be regarded as an elevator. But even the strongest elevator will break down if asked to lift too much weight.¹⁴

¹² John Spencer Bassett, "The Present State of History-Writing," in Jean Jules Jusserand, *et al.*, *The Writing of History* (New York, 1926), 117–18, cited in Novick, *That Noble Dream*, 172.

¹³ Letters on Handlin and Loewenberg, Arthur Schlesinger Papers: on Leopold, Merriman Papers, all cited in Novick, *That Noble Dream*, 172–73.

¹⁴ "Report of the History Department for 1956–7," July 15, 1957 (Griswold Presidential Papers), cited in Novick, *That Noble Dream*, 366.

Anxiety about the social origins of historians was anxiety not only about the elite status of the profession but also about the writing of history itself. This was clear in the presidential address to the AHA in 1962 by Carl Bridenbaugh. Bridenbaugh bemoaned “The Great Mutation” that had undermined the profession of history. Historians were no longer men who shared rural upbringings and the spiritual values that stemmed from the “common bond of Bible reading.” Instead, the profession was infected with the “virus of secularism,” and this was evident in the decline of manners and the end of “good taste” in the historical profession. Narrow specialists had replaced cultivated generalists, men with only book knowledge of life had replaced men of action. Worst of all, from Bridenbaugh’s point of view, was the erosion of “a common culture” that made possible true understanding of “the past.”

Today we must face the discouraging prospect that we all . . . have lost much of what . . . earlier generations possessed, the priceless asset of a shared culture . . . Furthermore, many of the younger practitioners of our craft, and those who are still apprentices, are products of lower middle class or foreign origins, and their emotions not infrequently get in the way of historical reconstructions. They find themselves in a very real sense outsiders on our past and feel themselves shut out. This is not their fault, but it is true. They have no experience to assist them, and the chasm between them and the Remote Past widens every hour.

Note Bridenbaugh’s assumption here that “our past”—that is the past designated as important by white, Protestant American men—is the only past worth considering as history. Children of lower-middle-class or foreign origins, he says, have “no experience” to help them understand this past. Bridenbaugh does not suggest that they have a *different* experience, or a *different* perspective on history; he cannot conceive that their experience constitutes history too, for that would undermine the supposed universality of his own view. Instead, he insists that there is one past (“our past”) and only one way to recount it. “What I fear is that the changes observant in the background and training of the present generation will make it impossible for them to communicate to and reconstruct the past for future generations.”¹⁵

As a corrective to the situation, Bridenbaugh urged that graduate schools admit students only of “broad and ranging general culture,” those suitably educated by class as well as training. “We might as well revive the cry of the Covenanters at Tibbermore,” he said in a revealing metaphor, “Jesus, and no Quarter.”¹⁶

Bridenbaugh’s attempt to disqualify members of the lower classes from the practice of history was framed as a defense of a universal, “true” story against misunderstanding by the ignorant. Yet the terms of his own discussion reveal that his notion of history was partial and particular. If it took a certain kind of experience to understand “our past,” was not that past then limited to certain kinds of people? Why was the story of those certain kinds of people, (“chaps,” Bridenbaugh calls them at one point) designated “history,” while that of others

¹⁵ Carl Bridenbaugh, “The Great Mutation,” *AHR*, 68 (January 1963): 322–23.

¹⁶ Bridenbaugh, “Great Mutation,” 328.

was irrelevant for understanding the past? Did the lower middle classes have no experience that could count as history? Did the children of immigrants have no sense of a past? From Bridenbaugh's perspective, only elites were sufficiently educated and disinterested to understand "History," only their values and standards ought to be applied to the story of the past. Indeed, although the story was said to be universal and therefore accessible to all, Bridenbaugh believed that only elites were prepared to appreciate its full meaning. Bridenbaugh's exclusion of "outsiders" from the profession suggests that he thought his version of history could predominate only if it were protected from potential challengers, from those whose different positions might lead them to other definitions, other values, other histories. The politics of this speech, in other words, had not only to do with access to the profession but also with the enforcement of a certain view about the meaning of history itself.

The current defenders of "traditional" history have no explicit objection to the presence of people from many backgrounds in the ranks of historians; indeed, many of them come from diverse backgrounds themselves. But they do seek to enforce the orthodoxy of a single standpoint, a single vision of what counts as historical knowledge. It is in that way that I read Theodore Hamerow's critique of "specialization" in the profession. He argued that some of history's transformation since World War II has resulted from an influx of new kinds of historians: "While previously history had been a preserve of patricians who often played an important role in political or religious affairs, now it became an avenue of upward mobility for people of scholarly ability but plebian origin."¹⁷ These people of plebian origin are later described (from a point of view he attributed to the founders of the AHA) as professors with "swarthy complexions" and "strange-sounding Celtic, Latin, or Semitic names."¹⁸ Their presence itself is not corrupting, says Hamerow (in contrast to Bridenbaugh, who thought it was), but the pressure on them to publish and succeed has turned history from a leisured activity into a frenzied "pursuit of profit," from a profession of generalists to a career for specialists. For Hamerow, there is an explicit connection between an increased representation of historians of plebian origin and increased specialization in the profession. And I cannot help hearing in this and other condemnations of specialization (which, like diagnoses of crisis, echo through the records of the American Historical Association) deep anxiety about challenges to elite versions of history.¹⁹ In fact, I would like to suggest that discussions of specialization are often (not always, but often) objections to democratization—to the appearance of varieties of interpretation, varieties of histories, and historians of diverse philosophies, all of which threaten the uniformity, continuity, and homogeneity that orthodox historians have traditionally sought to impose.

The current conservative claim to defend "History-as-it-has-always-been-written" is belied by the history of the profession itself. For there have been

¹⁷ Theodore S. Hamerow, *Reflections on History and Historians* (Madison, Wis., 1987), 77.

¹⁸ Hamerow, *Reflections*, 122–23.

¹⁹ See examples in Novick: on the 1920s, *That Noble Dream*, 190; on Allan Nevins's attack on specialization, 195–96. See also Crane Brinton's presidential address, "Many Mansions," *AHR*, 49 (January 1964): 31.

recurring challenges to orthodoxy, repeated contests about the meaning of history by scholars the discipline has come to recognize as fully accredited members of the profession. One of these challenges, the one most relevant to contemporary debates, stressed discontinuity, the relativity of ideas and values, group identities and conflicts, and (what Frederick Jackson Turner referred to in his presidential address of 1910 as) "Social Forces in American History." Above all, perhaps, this outlook has stressed the historicity of historical knowledge itself. An understanding of "the United States of today," Turner told his fellow historians, "demands that we should rework our history from the new points of view afforded by the present."²⁰ Two decades later, Carl Becker defined history as the meaning conferred on the past by the present, as "an artificial extension of memory," as "knowledge directed by purpose."

We are thus of that ancient and honorable company of wise men of the tribe, of bards and story-tellers and minstrels, of soothsayers and priests, to whom in successive ages has been entrusted the keeping of the useful myths. Let not the harmless, necessary word 'myth' put us out of countenance. In the history of history a myth is a once valid but now discarded version of the human story, as our now valid versions will in due course be relegated to the category of discarded myths.²¹

From this sense of the transmutability of historical knowledge followed the observation that historians were neither omniscient nor capable of completely mastering the entire meaning of past events. Charles Beard's 1933 presidential address, a critique of the notion of history as a totalizing objective science, was called "Written History as an Act of Faith." And Becker titled his speech, given two years later, "Everyman His Own Historian." The implication of both, explicitly elaborated by Becker, was that "history" consisted of many stories, all partial, all constructed to explain something, to "derive a satisfactory meaning." "It should be a relief to us to renounce omniscience," Becker wrote, "to recognize that every generation, our own included, will, must inevitably, understand the past and anticipate the future in the light of its own restricted experience."²²

In the 1930s and 1940s, historians influenced by the outlooks of Turner, Becker, and Beard devoted a good deal of attention not only to generational differences in approaches to history but to differences of experience and historical understanding within generations as well. Regional histories had already begun to challenge earlier emphases on the overwhelming importance of the northeast, but labor history, black history, and women's history also began to be tentatively explored. (In fact, all were represented in a session on "The Common Man" at the American Historical Association meetings in 1940.)²³ These topics also implicitly raised the question of the experience and standpoint

²⁰ Frederick Jackson Turner, "Social Forces in American History," *AHR*, 16 (January 1911): 226, 230.

²¹ Carl Becker "Everyman His Own Historian," *AHR*, 37 (January 1932): 231.

²² Becker, "Everyman His Own Historian," 235.

²³ Jesse Dunsmore Clarkson, "Escape to the Present," *AHR*, 46 (April 1941): 544–48. See also *Proceedings—1940* (Annual Report of the American Historical Association for the Year 1940), 21, 59.

of the historian: Could southerners write impartial history, it was asked. Could blacks overcome their “interested” position?

These questions were answered variously since historians who entertained so-called relativist ideas did not form a coherent school or group, nor were their political affiliations the same. They tended, however, to be less defensive about maintaining strict boundaries for history, more tolerant of philosophical disagreement. They might not like certain approaches to history, but they were less likely to condemn them as “not-history.” In addition, they did not take the notion of guardianship of professional standards to mean enforcement of homogeneity. At the 1940 AHA meetings, a group of “democrats” managed to secure the nomination of the first woman for the presidency of the AHA, and many of these same people were responsible for urging boycotts of southern hotels as meeting sites because these hotels refused to accept black guests. Writing in 1953, Howard K. Beale condemned discrimination in the AHA against “Negroes, Jews, Catholics, women, and persons not ‘gentlemen.’”²⁴ For him, there was a connection between democratizing the profession and loosening the hold of the guardians of orthodox history.

Although there were important differences among critics of prevailing practices, they were usually grouped together by the defenders of orthodoxy who sought to define them all as heretics, while singling out a few for particularly harsh (and exemplary) attack.²⁵ Beard and Becker were accused of “treason against the profession,” of promoting “defeatist” views that would corrupt the young, and of “unprofessional” conduct.²⁶ Peter Novick cited a review of Becker’s essays in 1935 that suggested that Becker was not a historian because his approach was “almost incompatible with usual historical procedure.” “Even in the studies which are most distinctively historical . . . he fails to show the conventional regard for events narrated in sequence.” Another reviewer reminded his readers that Becker was “not a ‘professional’ historian . . . He never took seriously the scholar’s paraphernalia of tools, or dug so intently among the dead facts that they came alive . . . the best of professional historians, who do their dissecting more deeply . . . do not end in Mr. Becker’s disillusionment.”²⁷ Critics of Beard’s opposition to American involvement in World War II linked his political views to his historical philosophy. Samuel Eliot Morrison found the same connection between Beard’s anti-interventionism and “Written History as an Act of Faith” as there was between “Adolf Hitler’s acts after 1933 [and] *Mein Kampf*.” And Oscar Handlin delivered the final disciplinary excommunication after Beard’s death. Handlin suggested that Beard was not really a historian:

²⁴ Howard K. Beale, “The Professional Historian: His Theory and His Practice,” *Pacific Historical Review*, 22 (August 1953): 235.

²⁵ On the divergent positions among “relativists” and “objectivists,” see Novick, *That Noble Dream*, 264–78. See also Richard Bernstein, *Beyond Objectivism and Relativism* (Philadelphia, 1983).

²⁶ Citations in Novick, *That Noble Dream*, 270–71.

²⁷ Stanley Pargellis, review of Becker, *Yale Review*, 25 (1935): 213–14, cited in Novick, *That Noble Dream*, 272. See also C. H. McIlwain, “The Historian’s Part in a Changing World,” *AHR*, 42 (January 1937): 207–24; and Allan Nevins, *The Gateway to History* (New York, 1938).

"He had no students . . . ; "His influence upon subsequent scholarship was slight."²⁸

There is a similar attack being launched today against all kinds of "new" history, however old some of its topics and epistemological positions. Gertrude Himmelfarb's polemic for this forum is a good example of the strategy. She lumps together in a single category of "deconstruction" all kinds of different approaches to history—those influenced by cultural anthropology, psychoanalysis, Marxism, behaviorism, feminism, structuralism, and poststructuralism—even though few of these have any connection to deconstruction at all. Even if they do not know it, she warns, historians taking these approaches are all "deconstructionists." She caricatures deconstruction as anarchy, rather than dealing with its philosophical analyses of language and metaphysics.²⁹ And she suggests that the arrival of "deconstruction" has removed "truth" as a ground for history, implying (wrongly) that debates about objectivity and relativism have never before existed among historians. In order to protect her particular position, Himmelfarb equates it with "history-as-it-has-always-been-written," but this requires repressing a good deal of the record of the past.

It also requires ignoring the diverse positions represented by the "new" history. Two of these positions are of particular concern these days. One has to do with the pluralization of the subject of history; the other with epistemology or philosophies of history. The multiplication of subjects and stories has necessarily raised epistemological questions, but not all historians who write women's history or African-American history or gay history or labor history answer those questions in the same way. Indeed, within any of these areas, there is disagreement among historians about what interpretive strategies to employ, the possibility of being objective, and the nature of the relationship between the historian and the subjects he or she writes about.³⁰ Conservatives have conflated all these differences into a single enemy because, in one way or another, they pose critical challenges to an increasingly embattled elitist, monolithic, and unitary conception.

The pluralization of the subject of history challenges the notion, dear to Himmelfarb and her associates, that "man" can be studied through a focus on elites. Instead, attention to women, blacks, and other Others demonstrates that history consists of many irreconcilable stories. Any master narrative—the single story of the rise of American democracy or Western civilization—is shown to be not only incomplete but impossible of completion in the terms it has been

²⁸ Samuel Eliot Morison, *By Land and By Sea: Essays and Addresses* (New York, 1954), 328; Oscar Handlin, *New York Times* (November 17, 1968): 28; and Oscar Handlin, *AHR*, 67 (October 1961): 148. All cited in Novick, *That Noble Dream*, 292.

²⁹ For serious discussions of "deconstruction," see Jacques Derrida, *Of Grammatology*, trans. Gayatri Chakravorty Spivak (Baltimore, Md., 1976); Christopher Norris, *Deconstruction: Theory and Practice* (London, 1982); Sande Cohen, *Historical Culture: On the Recoding of an Academic Discipline* (Berkeley, Calif., 1986); Lionel Gossman, "History and Literature: Reproduction or Signification," in *The Writing of History*, R. Canary and H. Kozicki, eds. (Madison, Wis., 1978); and Michael Ryan, "The Marxism-Deconstruction Debate in Literary Theory," *New Orleans Review* (Spring 1984): 29–35.

³⁰ Evidence of disagreement abounds in journal articles and book reviews especially. To follow controversies in women's history, to take only one example, one could consult recent issues of *Signs*, *Feminist Studies*, and *The Women's Review of Books*.

written. For those master narratives have been based on the forcible exclusion of Others' stories. They are justifications through teleology of the outcomes of political struggles, stories which in their telling legitimize the actions of those who have shaped laws, constitutions, and governments—"official stories."

The proliferation of Others' histories has not so much "politicized" the discipline (a charge usually leveled by the defenders of orthodoxy) as it has exposed the politics by which one particular viewpoint established its predominance. As such, it has raised questions about difference and power: how have differences among groups been constructed to organize and legitimize social and economic distinctions? How has the exclusion of some stories from the record of the past perpetuated inequalities based on attributions of difference? What is the connection between contemporary social hierarchy and measures of importance in historical writing? Answers to these questions call attention to the interested nature of history writing and undermine claims by orthodox historians that there is only one right way to tell a story, that their history is but a transcription of how things really happened in the past.

The notion that history is written by "the historian" has been pluralized as well; references to histories and historians indicate the multiple and heterogeneous nature of the profession. They also refuse the assimilation of all these different types into a singular position or point of view. This means that members of organizations like the AHA must accept the fact that professions are not homogeneous bodies but are made up of diverse constituencies contending for power and influence. ("Politics" is, then, not the antithesis of professionalism but its expression.) And it means, as well, that there is no single standpoint we can expect from historians. How then do we understand the relationship between the historian's identity and the group he or she writes about? Do women have a privileged relationship to women's history? Can whites write black history? Is "orientalism" an inevitable feature of First World accounts of the Third World? Is history a pluralist enterprise in which any assertion is acceptable, in which "anything goes?"

These questions underscore the fact that history is an interpretive practice, not an objective, neutral science. To maintain this does not signal the abandonment of all standards; acknowledging that history is an interpretive practice does not imply that "anything goes." Rather, it assumes that discursive communities (in this case, of historians) share a commitment to accuracy and to procedures of verification and documentation.³¹ These are not, however, invariant but are subject to shifts and changes. It also acknowledges that the meanings attributed to events of the past always vary, that the knowledge we produce is contextual, relative, open to revision and debate, and never absolute.³² We may be in a particularly heated period of interpretive conflict at the moment, but there is nothing unprecedented or unhistorical about conflicts of this kind; indeed,

³¹ On "communities of interpretation," see Alasdair MacIntyre, *After Virtue* (Notre Dame, Ind., 1981); and Stanley Cavell, *The Claim of Reason* (Oxford, 1979).

³² Natalie Zemon Davis offers an eloquent discussion of history as an interpretive practice in "'On the Lane,'" *AHR*, 93 (June 1988): 572–603.

historical inquiry is renewed and new lines of investigation opened precisely through such moments of intense debate.

Discussions of interpretation pose choices about which procedures, methods, and theories historians should rely on, and they also introduce questions about the nature and extent of the historian's own mastery. The most radical conclusions drawn about mastery are not far from Becker's call to "renounce omniscience," although today they rely on poststructuralist theories of "the subject." These theories link the writing of history to the construction of a continuous, centered "self"—the "self" of Western subjects and the "self" of the historian. They suggest that claims to mastery and objective knowledge are part of the ideological work of subject constitution.³³ And they insist strongly on the need for historians to reflect critically on the effects of their writing and to recognize the partial and relative standing of any work they produce.

Among the historians Himmelfarb lumps together as "deconstructionists," there is enormous debate on the question of "the subject." Indeed, I would say that most historians of "Others" are dubious about critiques of the subject and hostile to poststructuralist theory generally. They stress instead the need to depict the "agency" of individuals and groups and refuse the kind of critical reflexivity engaged in by many more literary scholars. Their stance is perfectly consistent with the rules of the discipline; it is that of interpreters of transparent evidence, scholars reporting on material they have dispassionately contemplated. Still, they attract the animus of conservatives, who would place them outside canonical history. Why?

I think that engagement in a democratized historical practice calls into question orthodox notions of objective mastery by fragmenting historical vision into conflicting accounts of what happened in the past. There is no denying the partiality and the particularity of the stories, and, by extension, of all stories historians tell. It is finally the plurality of stories and of the subjects of those stories, as well as the lack of any single central narrative that conservatives find intolerable because it undermines the legitimation of their quest for dominance. Their defense of their subject—elites in the past, their own hegemony in the present—is a repudiation of the possibility of contest and conflicting interpretation, a refusal of change, and a rejection of the possibility for what I would call democratic history.

My version of democratic history would accept the facts that there will always be a plurality of stories, that telling them involves contests about power and knowledge, and that the historian's mastery is necessarily partial. These statements raise difficult, but not insoluble, questions for the discipline: If the many different stories of the past, based on different historical experiences, are indeed irreconcilable, is there nonetheless a way to think coherently and systematically about the past? What are the contemporary social and political implications of

³³ Michel Foucault, *The Archaeology of Knowledge*, trans. A. M. Sheridan Smith (New York, 1972); Gayatri Chakravorty Spivak, "Can the Subaltern Speak?" in *Marxism and the Interpretation of Culture*, C. Nelson and L. Grossberg, eds. (Urbana, Ill., 1988), 271–313; Mary Poovey, *Uneven Developments: The Ideological Work of Gender in Mid-Victorian England* (Chicago, 1988), 1–23.

seeking such coherence? How can we maintain a disciplinary organization, with some commitment to shared standards and at the same time tolerate diversity in membership and profound differences in method, philosophy, and interpretation? What would a genuinely democratic history look like?

These questions are answerable, but only if we accept the notion that history itself is a changing discipline—as it surely is and always has been. They are questions that have opened for some of us far-reaching and enormously creative inquiries. They engage with some of the major philosophical debates of our age; indeed, they are an integral aspect of those debates. In that sense, they are part of the inevitable renewal of history in relation to contemporary life. Those who would preserve orthodoxy against this renewal threaten the vitality of history. Those who pose critical challenges are not enemies but agents of renewal and change. Those who would write “politics” out of professional life misunderstand the processes by which all knowledge, including knowledge of history, has been produced. Those who expect moments of change to be comfortable and free of conflict have not learned their history.

Perspectives on "The Old History and the New":
A Comment

JOHN E. TOEWS

IT IS DIFFICULT TO IMAGINE what an appropriate comment for a series of critical, polemical position papers of this kind should look like. I have received a number of suggestions. One was that I assume the role of a referee, point out fouls perpetrated by either side, add up legitimate points scored, and declare a winner. Another was that I might produce a sound-and-light laser show in which color-coded perspective beams would ultimately merge into a single spectacular image of harmonic convergence. However, I have remained enough of a traditionalist to resist such suggestions that the historian's task could be reduced to either a struggle for professional victory in a zero-sum competition, or the projection of wish-fulfilling fantasies on the past. Instead, I would like to confront and rethink the positions already presented in terms of both connections and differences that cut across the most obvious divisions between representatives of the old history and the new. My remarks will focus on the theme, explicitly or tacitly addressed by each of the papers, of the relationship between power, or a very broadly defined "politics," and historical knowledge, or our understanding of the past. What I would like to suggest is that two rather different conceptions of this relationship have informed the discussion thus far. The first, which predominates in the papers of Theodore Hamerow and Joan Scott, conceives power and knowledge in terms of domination, control or mastery of the object, in this case, the "past," a control that can extend along a spectrum ranging from recognized competency in shaping the inherited historical record to a full definition of the boundaries of the significant past and even the constitution of its essential nature. A second conception of the relationship between power and knowledge, predominating, though less explicitly, in the presentations of Gertrude Himmelfarb and Lawrence Levine, emphasizes the inter-subjective dimension of both power and knowledge. It conceives control or mastery in terms of a self-imposed order of laws or meanings that emerges as a consensual agreement from the interactions of autonomous historical actors or "subjects," an order that the historian encounters as already made.

Hamerow has presented us with a classic Weberian account of the bureaucratic rationalization of the vocation of history, of the transformation of a

learned society animated by missionary zeal and messianic hopes into an organized, institutionalized profession in which, to quote Weber directly: "The idea of duty in one's calling prowls about in our lives like the ghost of dead religious beliefs."¹ This general model for conceptualizing the historical process of professionalization has been around for a while and its components and consequences have become familiar: first, the expropriation and monopolistic control of the historical object, as well as the resources and means for its production and reproduction, by a cadre of trained and certified experts; second, the apparently inexorable division of labor and specialization in production driven by the need for greater technical competence and professional mastery; third, the restriction of the communicative boundaries of historical knowledge to other experts in increasingly narrow subdisciplines, or in a less reciprocal and more diluted form, to the passive, captive consumers in university classrooms; fourth, the growing separation of the professional reconstructions of the past from the hates and loves, fears and hopes of the non-expert members of the community or "general public"; and, finally, the apparent triumph of a narrow, internally defined technical competence over the broader, traditional calling of historical writing as a guide—ethical, prophetic, and aesthetic—to "life." The certified, competent academic seeking professional advancement has replaced the prophet/bard searching spontaneously and intensely in the collective memory for the meaning of life.

In Hamerow's analysis, the politics of professionalized history appear as peculiarly anonymous, systemic and structural, rather than individual or partisan, as destroying subjectivity, passion, commitment, and struggle, rather than expressing or exemplifying them. Such a vision of technical reason pursuing its own logic of efficiency without regard for individual or communal purposes undoubtedly continues to resonate in our consciousness. Weber is not yet a dead dog whose carcass can be relegated to the historical dustbin. However, even if professionalized historians have in fact become specialized cogs in a bureaucratic machine, their life activities would not necessarily isolate them from the dilemmas and concerns of their cultural contemporaries, or the general patterns of communal life but, rather, would constitute a particular, exemplary form of living those patterns, experiencing those concerns. The view through the bars of Weber's "iron cage" may very well be closer to "life" than the view from the gilded cage of the patrician amateur's salon. It is also worth noting that, in our own day, the majority of nonacademic historians are not in fact leisured spiritual aristocrats taking a respite from the world of affairs, but "public" historians in government agencies and private corporations, as intimately exposed to the processes of bureaucratization as any academics, perhaps more so. A more important criticism of Hamerow's conceptualizations, however, is its restriction of the politics of history to the external relations of professionals to amateurs and producers to consumers. It does not adequately recognize the political contest within the profession itself or the ways in which a focus on competency

¹ Max Weber, *The Protestant Ethic and the Spirit of Capitalism*, trans. Talcott Parsons (New York, 1958), 182.

or objectivity can disguise a partisan struggle to exclude or control specific groups or perspectives. This struggle is at the very center of Scott's paper.

Moving from Weber to Foucault, as we would say in my own subdisciplinary historiographical ghetto, Scott turns the problem of the relations between power and knowledge in a new direction. The definition of the true object of significant historical knowledge, of "objective" or "real" history, as well as the identification of the proper procedures for finding and representing this object, she contends, are the products of political conflict, of struggle among different groups and their different perspectives for control of the public or common past. Apparent agreement at times in the past about what history really is and confidence in its unity and narrative coherence, she suggests, derived from the ability of specific groups to control the terms of discourse and assert their perspective as universal and immutable. Challenges to such orthodoxies may take two general forms; the challenge of an excluded group with a specific unified historical perspective that could simply displace the old (this would seem to be the case in most traditional generational crises within a fairly homogeneous cultural elite or in the struggle between liberal and Marxist historians over the past century), or a challenge (and this appears to be the case in the most recent crisis) to the very idea of a single coherent, homogeneous history on the basis of a permanent affirmation of heterogeneity, multiplicity and perspectivism. While Hamerow sees the democratization of the profession as accelerating its inherent tendencies toward technical competence and specialization, Scott views democratization as producing a divergence of purposes, meanings and commitments; not as an ivory tower turning away from the burning issues of contemporary life but as a renewal and ferment grounded in increasingly intimate associations with precisely those issues.

Both Scott and Hamerow analyze the practice of historians in terms of the desire to dominate, possess, control the historical object. In both cases, however, the analysis is sustained critically by intimations of an alternative conception of power and knowledge. For Hamerow, the ideal of non-bureaucratic historical practice—the prophet/bard who speaks from within the experiential immediacy of the life of his community and is able to reconstruct stories that resonate with the universality of that experience and thus edify and instruct his contemporaries—is part of a rapidly disappearing and perhaps doomed past. Scott rejects such visions of socio-cultural homogeneity and the idealized master narratives that sustain them as the narrow, imposed perspectives of restricted traditional elites. Instead, she looks, cautiously and skeptically, to the possibilities of a critical democratic future, where the acceptance of the heterogeneity of the past and of different perspectives in the present might somehow lead to relations among historians and between historians and the past that would not be defined by domination or exclusion.

Such alternative conceptions of the relations between power and knowledge, which operate as a critical background in the analyses of Hamerow and Scott, emerge to the foreground in the papers of Himmelfarb and Levine. What Scott projects into an imagined future in which inter-subjective agreement is secured on the ground of equal power and general autonomy won in contest and

struggle Himmelfarb would have us believe is already there to be found and reconstructed as our inheritance from the past. Although Himmelfarb is certainly conscious of the reality of politics as a struggle for domination over the desired object and over the competitors for it, she insists that politics is also, and essentially, the transcendence of this struggle and “the rational conscious deliberate attempt to organize public life so as to promote the public weal and the good life.”² This practical political rationality, directed toward the formulation and realization of public ends, rather than greater efficiency in the means of domination, is embodied in constitutions, laws, political institutions, and the publicly articulated, and thus fully documented, arguments of political leaders and political philosophers. The history of political reason is also the history of freedom, a master narrative of autonomous individuals coming to agreement about the purposes of their societies and the appropriate means for pursuing those purposes. It is also the only real “total history” (as opposed to the pseudo *histoire totale* of the Annalists or the deconstructionists) in the sense that the public reason of politics provides the ordering context for all individuals in a community, including those who have no active role in its production and reproduction. Moreover, Himmelfarb insists that recognition of the rational substance of past politics grounds the ideal of historical objectivity. Practicing historians in the present, insofar as they themselves are rational and autonomous, can confidently engage in the tasks of reconstructing the publicly documented history of reason and freedom in the past, and transmit this knowledge to later generations so they may comprehend the reason incarnate in their inherited public institutions and political traditions. Objectivity here is defined not as an adequate representation of the object but as inter-subjective communication and the possibility of consensus among rational beings, among the dead, among the living, and between the living and the dead.

From this high ground, Himmelfarb has consistently criticized the “new” history in all its myriad forms as a root-and-branch attack on politics, reason, and freedom; as a form of determinism in which individuals are dissolved in the anonymous structures of climate and demography, economics and social relations, unconscious collective mentalities and linguistic discourses; as a retreat from the great unifying issues and events of public life to an obsessive interest in the private and local dimensions of human experience, from sex to popular entertainment; and, finally, as a rejection of rational communication and the possibility of consensual objectivity through the affirmation of an epistemological relativism (and its correlate—the absence of any real historical object) in which all perspectives are proclaimed as equally legitimate.

Levine’s paper unveils the narrow basis and restricted scope of Himmelfarb’s perspective on the historical past and historiographical present, her identification of the significant past with the actions and words of tiny political and cultural elites, her identification of the public life of communities with the perspective of those who wielded public power and controlled the discourse of

² Gertrude Himmelfarb, *The New History and the Old* (Cambridge, Mass., 1987), 18.

official culture. However, Levine also uses Himmelfarb's ideal of inter-subjective community and self-imposed collective meaning to undermine her own claims. He sees the new history not as a rejection of politics, reason, and freedom but as an expansion of their scope. He extends the circle of significant historical actors, of the producers of meaningful discourses and communal institutions that embody conceptions of collective purpose and the common good, beyond the narrow confines of traditional elites to those groups and individuals who previously appeared from within the perspectives of these elites as silent, impotent others, as objects rather than subjects of meaningful history. This historical enfranchisement of the previously excluded as active makers and not just victims of history, as producers and not just consumers of cultural meanings whose experiential and symbolic worlds must and can be understood on their own terms, constitutes the great and lasting achievement of the new history. But it is important to remember that, even among opposing representatives of the new and the old, like Levine and Himmelfarb, there persist significant areas of continuity and agreement in theoretical assumptions and moral values, an agreement broad enough to sustain, however heated and confrontational, a meaningful dialogue, perhaps even a rational discourse.

In this comment, I have tried to suggest that two different conceptions of the relationship between power and historical knowledge predominate in differentiated ways in the papers presented and that these differences cross over the more apparent ideological confrontations between the old and the new history. I would add that both conceptions are one-sided abstractions that require the other to maintain their critical power in illuminating the past. Levine's conception of the national community as a complex, heterogeneous compound of autonomous communities, each of which must be understood from the native's point of view, clearly demands, as its necessary complement, a healthy cynical awareness of the warping, oppressive, corrupting impact of hierarchical systems of cultural and political domination if it is not to collapse into an illusory vision of everyone autonomously cultivating his or her own cultural garden in pluralistic harmony. But Scott's conception of power as a conflictual relationship of domination and subordination also requires a heightened consciousness of the (however limited) possibilities of dialogue and consensus if it is not to fall into a despairing vision of endless repetition in which the faces of masters and slaves may change, but the master/slave relationship remains unchanged. The point is that the presentations we have heard do not articulate incommensurable perspectives but arguable positions in a meaningful dialogue. They are part of a historical discourse about the nature of the past and its relationship to the present that possesses significant elements of continuity with the often conflicting but sometimes complementary relationship between two traditional master narratives connecting memory and hope that can be traced back at least as far as the eighteenth century: the story of the struggle for emancipation and the story of the struggle for social integration and cultural unity. The expansion of these stories to include those previously defined as strangers in our midst or "others" beyond our borders will undoubtedly entail the production of more

complex interactive and open-ended narratives, but to equate such expansive revisionism of the stories of liberation and inclusion with the collapse of historical coherence is simply to assume the universality of the perspective of those for whom liberation is grounded on domination and citizenship entails pledge and exclusion.

What then constitutes the contemporary crisis in history? Peter Novick's already cited history of the American historical profession concludes with this striking judgment:

As of the 1980s sensibilities were too diverse to be gathered under any ecumenical tent. As a broad community of discourse, as a community of scholars united by common aims, common standards and common purposes, the discipline of history had ceased to exist . . . The profession was as described in the last verse of the Book of Judges: "In those days there was no king in Israel: every man did that which was right in his own eyes."³

Similar apocalyptic passages increasingly light up the normal prosiness of contemporary historical writing. But the ease with which anyone can find apt apocalyptic images or citations in almost any cultural tradition or historical period should also serve as a warning, especially to historians. Human culture and even the practice of writing history have survived the deaths of kings and gods, and numerous centers that could not hold. The question facing us is whether or not the collapse of a certain kind of consensus, a consensus based on homogeneous social experience and political assumptions, on a shared language, common values, and identification with a single master narrative of the significant past, also implies that "mere anarchy is loosed upon the world." The deconstructionist critique of the organizing categories, assumptions, and language that have been used to justify and universalize a particular perspective on the past marks the "end of history" only for those who inhabit that perspective without a historical consciousness of its limited, temporally and culturally situated, constructive and contingent nature. This critique, however, is also applicable to those alternative, adversarial positions that insist on their own ahistorical essentialism, autonomy and incommensurability. The heterogeneous historical experiences and cultural perspectives that ground the new history (or histories) are after all related to each other and to the official, unitary history they confront in specific, historically identifiable, empirically reconstructable ways. The connecting ties are horizontal, secular, contingent, dynamically changing and proliferating lines of opposition and reciprocity, of domination, subordination, and mutual recognition, not vertical lines with predetermined trajectories that merge in, or derive from, a common metahistorical essence or center. The empirical reconstruction and critical extension of these lines of connection, not from one privileged and dominating center but from many different locations within the open-ended web of relations, would seem to be the appropriate task of a potentially democratic and global history, a history in which all human beings would be recognized as active makers of their own history, even as they suffer and are molded as objects in the history of others.

³ Peter Novick, *That Noble Dream: The "Objectivity Question" and the American Historical Profession* (New York, 1988), 628.

Reviews of Books

GENERAL

GERTRUDE HIMMELFARB. *The New History and the Old: Critical Essays and Reappraisals*. Cambridge: Belknap Press of Harvard University Press, 1987. Pp. 209. \$20.00.

In these essays, published as individual pieces between 1974 and 1986, Gertrude Himmelfarb reasserts familiar conservative arguments in support of "traditional" history. That history, she says, takes politics as its subject, narrating the progress of "man's" reason as expressed in his creation of the laws, constitutions, governments, and nations, which through "the rational ordering and organization of society" (p. 21) "promote the public weal and the good life" (p. 18). Traditional history, in her account of it, accommodates change without disruption precisely because change is made part of a continuous story. It is, moreover, a history that grants the reality and knowability of the past, seeks to describe it objectively and to transmit a "patrimony" that, from one generation to the next, secures the continuity not only of national identity but also of Western civilization. This history constitutes the right way of telling the human story, Himmelfarb suggests, because it accurately describes the way things actually happen.

Himmelfarb assumes that the objectivity of traditional history is as self-evident as the rationality of "man," and so she rarely engages in a defense of these propositions. Those expecting serious philosophical arguments will be disappointed, for there is little depth to these essays. They have a learned tone but not much of the substance that scholars expect; they seem to be aimed at rallying to her viewpoint educated elites in a position to exert influence on social policy (what the nineteenth century called "enlightened public opinion"). Rather than elaborating her own position, Himmelfarb simply states it and then attacks its adversaries, building her case through negative contrasts. This way of proceeding is best characterized

as polemical, even ideological, and it ultimately undermines the author's claims for the neutrality, objectivity, and truth of her position.

Himmelfarb's animus is directed against the dominance of the "new" history, actually a variety of approaches to the study of the past that are not necessarily the same but have together brought about a "revolution in the discipline" (pp. 23–23). These "new" interpretations, Himmelfarb insists, deny the drama of events, the power of ideas, and the dignity of individuals by ascribing to them causes and motives other than those explicitly invoked or avowed. Psychology's emphasis on the unconscious, desire, and passion, social history's attention to hierarchical structures and long-term processes of change, anthropology's concern with the formative cultural power of ritual and symbol, Marxism's theories of economic interest all involve for Himmelfarb a "radical," and therefore unacceptable, redefinition of human nature and thus an abrogation of the proper subject of history.

The terms "revolution" and "radical," with their implications of subversion, substitute for serious engagement with these theories. Why is politics better thought of as an exercise in restraint of self-interest (p. 20) than as an exercise in power or domination by one group over another? Why ought we to accept ideas as literal, intentional utterances of individuals rather than as variations on cultural themes or vehicles for furthering collective interests? Is progress the best way to characterize the human (or even the Western) story? Is it clear that human beings are the rational masters of their destinies? Is humanity best represented by the singular "man"? Is politics necessarily the best evidence of rational activity? Have historians "traditionally" assumed that was so? Although these complex questions have been debated by philosophers and historians for generations, Himmelfarb reduces them to a simple choice: for or against politics as rationality, the old history or the new.

In Himmelfarb's presentation of it, moreover, there is only one choice, since those who embrace the new history have ulterior motives. Their his-

tory is said to be tainted by contemporary outlooks that compromise objective understandings of the past or by contemporary political allegiances that demean and denigrate the old history in a kind of revenge for the failure of Marxism as a theoretical and political movement (p. 16). British Marxists take the brunt of her attack, their studies of England dismissed out of hand because, she says, they have never written histories denouncing Communism or Stalin. But other new historians are represented as guilty, too, because (at least rhetorically) she associates their present-mindedness with Marxism. "In a sense the new social historian goes even further than the Marxist" (p. 16). "For the social historian, however, as for the Marxist . . ." (p. 17).

The alternative is the high-minded "moralism" Himmelfarb attributes to "great" or intelligent thinkers: Disraeli and Carlyle in the nineteenth century, Geoffrey Elton and Robert Nisbet in the twentieth. By definition, she says, their work is beyond ideology. And yet one wonders if such assertions can be maintained in the face not only of what "old" historians have written about the Victorians she cites but also of a quotation she offers as exemplary from Nisbet's *History of the Idea of Progress* that includes as one of his five "crucial premises" the "conviction of the nobility, even superiority, of Western civilization" (p. 169).

The association of rationality, progress, and objectivity with a defense of the superiority of Western civilization suggests that Himmelfarb's "traditional" history is, in fact, official history—a story told to legitimize the actions of those in the West who have shaped laws, constitutions, and governments. It suggests, further, that the "new" history is perceived as dangerous by the "old" because it offers a critique of that official story and explores other ways of telling it. The confrontation of the "old" history and the "new" is then not, as Himmelfarb would have it, a battle between truth and its enemies. Rather, as this book clearly demonstrates, claims to "truth" and to the sanctity of tradition are not innocent of contemporary political allegiances. The contest between the "new" history and the "old" is a contest about the uses and meanings of historical knowledge. In that sense, it could be concluded that, even if history is not always about politics, it is in some sense inherently political.

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CHRISTIE FARNHAM, editor. *The Impact of Feminist Research in the Academy*. Bloomington: Indiana

University Press. 1987. Pp. ix, 228. Cloth \$27.50, paper \$12.95.

Does most contemporary scholarly research on women deserve the label "feminist"? Such is the implication of the title of this collection and of editor Christie Farnham's first sentence, which characterizes women's studies programs "as the academic arm of the feminist movement." Farnham introduces and organizes eleven essays, most of which were commissioned by Indiana University's Women's Studies Program. Certainly, the decisions of numerous scholars since the late 1960s to focus their research on women occurred as American feminism was again on the rise and spurring academic women (and some men) to ask why the human experience studied by the traditional disciplines of the humanities, social sciences, and biological sciences was so often limited to men's experience. A scholar's decision to study aspects of women's lives can be labeled a "feminist" step in that the choice represents an effort to provide the "equal" treatment of women and men that feminism seeks. Yet this reviewer (who is a feminist) believes that just as not all studies of socialism and socialists should be characterized as "socialist" research, so, too, not all research on women is "feminist."

The quibble over the title should in no way diminish this collection's value for historians interested in learning more about the impact of research on women in the disciplines of anthropology, biology, economics, literature, political science, psychology, religious studies, and sociology as well as in history. The essays do not have identical focal points, however. They are grouped into four categories: "The Articulation of Gender as an Analytical Category," "Methodological Moves from Margin to Center," "The Sticking Power of Stereotypes," and "Paradigmatic Implications." The treatments of the anthropology of women by Louise Lamphere and of the history of women in modern Europe by Joan Scott well demonstrate the importance of gender as an analytical category by providing both a broad overview of important questions raised by researchers and an excellent introduction to bibliography. On the other hand, Nellie McKay's essay on black women writers is primarily a presentation of texts written since 1746 that deserve study and challenge stereotypes. Political scientist Virginia Sapiro limits her discussion to assessing how new research on women has influenced the study of political socialization, while economist Barbara Bergmann combines an analysis of why women have been paid less than men with a call for feminist economists to help achieve equity. Psychologist Carol Gilligan sets forth her much pub-

licized argument about how differences in men's and women's formulation of moral values challenge the validity of any single model of human psychological development.

Historians who did not read Joan Scott's essay when it first appeared in *Past and Present* in 1983 will be interested in her division of recent work on the history of European women into three categories, the first two being preludes to the third. The first category, "herstory," includes a compensatory search for heroines, an effort to fit women into the standard outlines of political history, and also studies of ordinary women for whom the "personal" was as important as the "political." The second variety draws heavily from the ascent of social history, employing quantitative methodology and interdisciplinary insights and also emphasizing, either with or without Marxist theoretical underpinnings, the importance of the experience of groups typically neglected in traditional histories. The weakness of much of the social history of women, according to Scott, is a primary emphasis on economic factors rather than gender which leads, in turn, to findings that are too "integrationist." Nonetheless, both types of efforts to include women in history have contributed to a reevaluation of standards for assessing historical significance and also demonstrated that developments typically viewed as progressive for men—such as the Renaissance, technological innovation, democratic revolutions, and the "affective" nuclear family—often imposed new constraints on women. Scott's third and most mature category endeavors to relate gender to the workings of political and economic power and to stress the interrelationships of men's and women's lives. Many historians can acknowledge the reasonableness of this goal, despite Scott's attachment of "radical potential" to it. One may ask, however, whether existing work in this third category is quite so limited as Scott suggests. For example, Steven Hause's fine study of the French women's suffrage campaign is placed by Scott within the "herstory" phase, but other readers might well assign it a place within the sophisticated third category of focus on interrelationships between gender and power.

Although Scott's essay is the only one by a historian, historical perspectives inform other essays. Thus Lamphere notes that some West African women traditionally played a role as providers that was as important or more important than that played by men. She also addresses the impact of imperialism on the colonized and challenges anthropologists to produce more "concrete, specific, and historically sensitive studies" so that women do not appear as an undifferentiated group. Carol P. Christ argues that the study of prehistoric

goddesses of the ancient Near East and Europe is producing a "paradigm shift" in religious studies. An insightful chronicle of reasons for American sociologists' well-known practice of ignoring women provides the prelude to Jessie Bernard's comparable conclusion that the new focus on women is transforming more than one discipline.

In sum, the essays in this collection certainly confirm Farnham's optimistic assertion that in many academic disciplines "feminist research" is moving from "ghettoization" to the mainstream of scholarly concerns.

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ALICE BROWNE. *The Eighteenth Century Feminist Mind*. Detroit: Wayne State University Press. 1987. Pp. viii, 237.

This is essentially an old-fashioned "history of ideas" treatment of a new-fangled subject, eighteenth-century feminism. It is likely that this book will appeal more to traditional literary scholars than to historians, who will prefer something more analytical than this litany of ideas provides. Unfortunately, *The Eighteenth Century Feminist Mind* too often reads like an unrevised doctoral dissertation, in which the author seems unaware of the significance of her own material and consequently does not know what to do with it. There is little sense of direction from chapter to chapter, and the book ends without ever concluding: I was personally shocked to turn page 177 and find the End Notes staring me in the face. Nothing in the book had prepared me to think that it was coming to such an untimely end. Indeed, I had continued to hope that a final, concluding chapter would make sense out of the jumble of ideas that had been hitherto presented.

The thesis of Alice Browne's book is that there is a relationship between eighteenth-century ideas about women in general and eighteenth-century feminist ideas. The point may seem an obvious one to some readers, but it is nevertheless an important and interesting one. However, it is not developed properly. Browne does not tell us what, if anything, differentiates this relationship in the eighteenth century from earlier or later periods. Although the influence of Locke and the development of political theory since 1688 is frequently referred to, we never get any overall evaluation of these factors that may have led to the growth of a feminist mind in this particular period.

Browne devotes most of the book to a comparison of feminist and non-feminist ideas on particular subjects, such as the sexual double standard or the question of education for women. Most of

the writers quoted by Browne were labeled by her as either feminists, misogynists, or moralists. She describes other writers, such as the novelist Samuel Richardson, as making feminist points without themselves being feminists. What this reader needed most of all were clear definitions. What does the author mean by "feminist"? Are we using an eighteenth-century or a twentieth-century definition? What body of literature comprised the eighteenth-century feminist mind? With what body of literature shall we compare it? The structure of *The Eighteenth-Century Feminist Mind* provides little clarification. The first part of the book ostensibly deals with general notions of women in the eighteenth century, while the second part, sensibly enough, purports to be the feminist responses to these ideas. But, in fact, Mary Wollstonecraft and other feminists show up in the first part of the book, while such "moralists" as John Gregory and Hannah More appear in the second. So we are never sure what is being compared with what. One of the more interesting observations a reader might make is the frequency with which there is a coincidence of ideas between feminists and moralists, and one wishes Browne had developed this relationship.

The failures of this book are perhaps not the author's alone. An experienced editorial staff might have helped a (presumably) inexperienced author avoid some pitfalls. Each chapter's statement of purpose comes at the end of the chapter, rather than at the beginning, where it might have been of use to the befuddled reader. The convoluted structure of many sentences begged for one more rewrite. Other problems that this reader experienced may come from my background as a social historian and may trouble only those similarly inclined. I wanted to know more about who these feminist authors were, their backgrounds and motivations, and how they may have differed from authors of a different stripe. I would have liked to know who read these works and what, if any, influence feminist authors had. But that would have been to write a different book. In fairness to Alice Browne, her title appropriately focuses on the feminist *mind*, and the work is, indeed, about that rather than about feminists or feminism more generally. As such, this book provides us with a richly textured and highly detailed account of a particular frame of mind that should be of interest both to students of feminism and to students of the eighteenth century. And, although Browne does not evaluate the contributions of eighteenth-century feminists, her work suggests the centrality of their thought to the period. It would be difficult indeed ever again to argue that

feminist ideas occupied a marginal position in eighteenth-century society.

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DOMINICK LACAPRA. *History, Politics, and the Novel*. Ithaca: Cornell University Press. 1987. Pp. ix, 217. \$22.50.

In this collection of short essays, Dominick LaCapra applies his brand of intellectual history to a series of "modern" novels: *Red and Black*, *Notes from Underground*, *Middlemarch*, *Sentimental Education*, *Death in Venice*, *Doctor Faustus*, *To the Lighthouse*, and William Gaddis's *The Recognitions*. The selection of texts might appear to be "aleatory," to use one of LaCapra's favorite words, but he uses them to illustrate what he considers significant problems in "reading" works of modern fiction. Scholars unfamiliar with LaCapra's other studies should be warned that this is not a work for beginners. Indeed, in many ways it reads like a continuation of *Rethinking Intellectual History: Texts, Contexts, Language* (1983). Moreover, many unsuspecting historians might accuse him of false advertising, since they will not find much about "history" or "politics" if they take the book's title in a conventional sense. LaCapra is probably weary of reading reviews of his works that question the degree to which he is engaged in historical research, yet I must confess that I still have the same queries.

Warning against both "overcontextualization" and "formalistic hypostatization" (p. 209–10), LaCapra claims that he wants to present interpretations that are neither "narrowly historicist (in reducing texts to mere documentary symptoms of contexts) or formalist (in isolating and remaining rigorously . . . within the internal workings of texts)" (p. 7). In practice, however, he lands very close to the second half of that presumed polarity. Although he alludes to historical contexts as well as the ex post facto "context" provided by literary critics, most of the conclusions that he reaches can be derived by "formalist" readings of the novels in question, as long as one's mind has been imprinted in advance by the works of Mikhail Bakhtin. Using that scholar's notions of the "carnavalesque" and the "dialogic" structure of novels, LaCapra assays the extent to which the novels "relate in symptomatic, critical, and possibly transformative ways to [their] pertinent contexts of writing and reading" (p. 4). His major point, following Bakhtin, is that modern novels betray multiple voices that call into question basic assumptions about society, the "bourgeois" individual (be that the hero, the author, or the reader), time, and other fundamental

categories of structuring experience. Modern novels include "carnavalesque" devices such as irony and parody that destabilize such notions and, therefore, are "potentially transformative"—and hence possibly political—insofar as they liberate the mind for a "free play" of thought and aspiration.

These ideas are interesting. But what do they tell us about the past as it was "actually" conceived and experienced? (Despite the increasingly sophisticated and self-critical nature of the historical profession over the past century, it is hard to purge a residuum of the old *wie es eigentlich gewesen* standpoint from one's blood.) LaCapra does not provide enough information about what he dubs "immediate contexts" (such as authorial intentions) to determine the extent to which the "dialogic" ruptures are conscious strategies or anything else. After all, a novel's "resisting fully concordant narrative closure" (p. 14) can also be a function of the limitations of language and literary convention, unconscious cracks in an author's world view, and even unintentionally sloppy writing. Moreover, one needs to ask (not to mention demonstrate) to what extent the "potentially transformative" implications isolated by LaCapra were appreciated by a novel's readers. Those might be questions of "immediate context," but I do not see how they can be isolated from a confrontation with the past.

Ultimately, it seems that LaCapra is most interested in confronting the present, in the shape of the historical profession, and even challenging the future. Although he claims that the past is not an "it" that can be either "neutrally represented" or "projectively reprocessed in terms of our own narrowly 'presentist' interests" (p. 10), such "presentist" interests abound in his own work. First, he wants historians to become better readers of texts by learning the insights of recent literary theory. Second, he believes that we can become better writers of historical texts if we comprehend the complexity of writing; by recognizing that novelistic narrative is not as straightforward as we might assume, we might question our own adoption of narrative techniques in our monographs. Third, he believes that an appreciation of the "contestatory" elements in "great texts" might make us more "contestatory" in our own lives and thus have a "potentially transformative" function; motivated by "a critical concern for the actual and desirable interaction of work and play in culture," he hopes "to facilitate an exchange between past and present that recognizes and intensifies our implication in processes of reading in ways that may have import for the future" (p. 210). The first two concerns deal with historiography, as well as epistemology; the third is an act of citizenship

rather than scholarship (in Weber's sense). The first two provide useful services to the historical profession; the third is a much more problematic enterprise. But, in any case, none of the three endeavors generates an account of historical processes.

LaCapra would clearly like to explain historical change and innovation as well, but, to do that, he needs to be less dismissive of historical approaches that he believes contradict, but in fact may complement, his own. I do not agree that intellectual historians, or historians more generally, are as "reductivist" or as ignorant of literary problems as he assumes. It is both ungenerous and unfair to suggest that the field is plagued by "relatively conventional accounts of contexts and equally conventional if not banal readings of texts" (p. 210). Quite a few historians are cognizant of the critical sources that inspire LaCapra but consider their utility limited; familiarity with those works does not necessarily breed conversion. A more inclusive rather than exclusive approach to new developments in intellectual history, literary theory, and—let us not forget—the social sciences of culture would seem more promising. To unite them would be a tricky act of intercourse, but it could produce a stunning hybrid, and LaCapra would be better able to graft his own insights onto it.

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RUDBOLPH BINION. *After Christianity: Christian Survivals in Post-Christian Culture*. Durango, Colo.: Logbridge-Rhodes. 1986. Pp. 136. \$6.50.

Although Rudolph Binion mentions psychohistory no more than a few times in his book, this is intellectual history informed by a psychohistorical perspective. The author's knowledge is immense, ranging over art, literature, theater, film, historiography, history of science, anthropology, and linguistics, and, as for scholarship and knowledge of languages, in three pages of endnotes to chapter 2 (pp. 63–65), he cites works in English, German, French, Italian, Spanish, Russian, and Danish, proving, incidentally, that there is such a thing as Western civilization and that there is such a thing as Europe, an area with a common core of both traditions and concerns.

Is there such a thing as Christianity? According to the author, there was, and it collapsed suddenly in the eighteenth century for reasons he cannot explain. Christ, who held the elements of Christianity together, disappeared. God was gone. As a character in Samuel Beckett's *Endgame* at once both affirms and denies, "'The bastard! He doesn't exist!'" (pp. 55–56). Three elements fell

loose and are played out in other forms in nineteenth- and twentieth-century history: the "Judgment [afterlife], the Fall and the Truth," three "Christian Fragments" (p. 120), without the Christ, Redemption, and Grace to hold them together. The collapse of Christianity must have been traumatic, the author concludes. "The return of the afterlife in disguise over nearly two centuries running, complete with its hard-fought repudiation, suggests . . . it was and remains, traumatic—a self-inflicted wound that cut to the quick without the wounded ever quite realizing it" (p. 58).

One hesitates to rush in where angels fear to tread. Binion is wise enough to interpret and describe without explaining. He never pretends to account for the collapse of Christianity or, in the "cosmic perspective" of the big bang, to give any "ascription of purpose to human acts" (p. 124). Yet he believes that the purpose of psychohistory is to deal with "the interplay of surface intentions and . . . concealed motives that . . . must yield to the nullity of both" (p. 124). Perhaps, but few of us are sufficiently stable or wise to rest content with the void, as Binion's study amply demonstrates with myriad examples.

A tentative explanation (not that of Binion) might suggest that the problem is one of the undoing of a constellation of linked archetypes. When the figure of a savior gave way (if it did; certainly it did for many intellectuals), the cultural complex held together by Christ floated off in at least three directions, but the peripheral archetypes did not go away, so deeply ingrained were they in the Western mind. If this is so, we have not seen the last of the manifestations of the undoing of this linkage and can expect further reappearances of the archetypes in other forms. Without using this terminology, Binion has, I believe, uncovered a central problem of the last two centuries and illuminated it for us brilliantly. We need not, however, accept his deeply pessimistic conclusions. At least I hope not.

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MICHAEL J. BUCKLEY. *At the Origins of Modern Atheism*. New Haven: Yale University Press. 1987. Pp. viii, 445. \$27.50.

This work offers a structure of how atheism became a system in modern thought. Starting from the skeptical crisis of the late sixteenth century, Michael J. Buckley contends that the threat to religion was first met by assigning its defense to philosophers and then to scientists, who finally constructed a world without God.

Two Catholic theologians, Leonard Lessius and Marin Mersenne, are portrayed as the first to conceive of the threat of atheism, before there were any genuine flesh-and-blood atheists, and Lessius and Mersenne made philosophical argument the way to combat it. They were followed by René Descartes, a philosopher, not a theologian, who proved the existence of God philosophically and derived nature from the philosophical deity. Then Isaac Newton and his disciples William Whiston and Samuel Clarke made nature the basis for proving God, science the base of theology. It was then a short step for the French materialists to combine Descartes's mathematical nature, Newton's science, and a vitalistic biology to create a system, in Paul-Henri-Dietrich d'Holbach's terms, that explained everything without any supernatural deity and that explained the Judeo-Christian world as a natural development within the human world.

Buckley's construction is well knit and sometimes amazingly well done, as in the section on Newton. The presentation of the views of the selected cast of characters is carefully documented from texts and some secondary studies. The work is sometimes overwritten and dramatized and becomes polemical advocacy of a certain kind of Catholicism.

Before the book becomes the accepted answer to Andrew White's *Warfare between Science and Religion*, I think one has to consider some of its basic drawbacks. Buckley sets forth a logical structure of ideas leading from the philosophical proposals of Lessius, Mersenne, and Descartes to the solid atheism of d'Holbach. This structure is detached from the historical realities of the two centuries covered. The philosophical and scientific theories are presented in ideological isolation, and Buckley keeps thundering that the philosophers and scientists ignored the reality of religious experience, Christology, and Scripture. First of all, I think it is Buckley who ignores the religious developments taking place at the same time—the Reformation, the splits within both Catholicism and Protestantism, the impact of Judaism on Christianity in the period, and the rise of Biblical criticism, comparative religion, religious relativism—developments that resulted from new historical factors, namely the voyages of exploration, the humanist study of texts, the impact of Jewish literature (especially the Kabbala), the religious conflicts, and the new forms of religious mysticism and religious expectation (millenarianism and messianism). Without some reference to the context, it is very difficult to account for why the learned world accepted the philosophical ideas on religion of Descartes, Newton, and others. There may have been a logical relationship between one

set of ideas and another. But why should anyone in the seventeenth- and eighteenth-century world have cared?

I think one could invert Buckley's picture and show that, because of various developments in religious affairs, a type of critical questioning of the authenticity and accuracy of the Biblical text occurred. Because of certain ideas of Reformers, Counter-Reformers, and Jewish critics of Christianity, a quite different consideration of Christian theology was occurring in Europe from the early sixteenth century onward. Over a century before d'Holbach, the possibility was being considered that religions are man-made and that Judaism, Christianity, and Islam were developed and maintained for human political reasons. The idea of *Les Trois Imposteurs*—Moses, Jesus, and Muhammad—was a much discussed theme in the mid-seventeenth century. Theorizing about theology went on in a world in which a wide variety of considerations was leading to post- or non-Biblical views. Most of what d'Holbach had to offer was presented by figures, such as John Bodin, Thomas Hobbes, Baruch Spinoza, Pierre Bayle, and the English deists, who hardly play any role in Buckley's account. The underground literature that was circulating all over Europe contained the most radical nonreligious ideas long before d'Holbach (and he published some of it in his edition of *Les Trois Imposteurs* and *Israel vengé*).

The course of development from the Age of Belief to the Age of Reason involved not just a succession of philosophical theories but also a changing religious context that needed explanation. The philosophical attempt to provide this explanation involved the best minds of the time. In selecting who is most significant, I think one can construct just as convincing a case for Spinoza or Bayle as for Descartes, Newton, and d'Holbach. But the problem still remains: why did anyone believe them? And that aspect is not treated by Buckley. He just bemoans and berates a world that did accept them. In so doing, he overlooks the so-called under-belly of the Enlightenment, namely, its religious side. While the system of atheism was being developed, various forms of modern religion were also emerging—quietism, spiritualism, pietism, Hasidism, fundamentalism, millenarianism, and cabalistic messianism. One could, and some did, call the period the Age of Revelation rather than the Age of Reason. The Great Awakening, the outpouring of millennial interpretation during the French and American revolutions, the spiritualized Judaism of the Hasids, the mystical Catholicism of the Jansenists, all represent a counter picture to demolition of religion by scientific humanism.

Not only did modern atheism develop between

1600 and 1800, so did many forms of contemporary religion, which have played and are playing a vital role in the modern world. Buckley's account and his dire evaluation of it must, I think, be reconsidered in the light of the actual issues that led to the breakdown of traditional belief and the forces positive and negative that created modern religious and irreligious ideologies. Buckley's book is most provocative. It has to be taken into account in forming a view of what has been involved in the making of the modern mind. But other accounts can be offered and supported with equally strong evidence. And the actual historical realities of the world of religious experiences of the time suggest much more has to be accounted for.

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PAUL VEYNE, editor. *A History of Private Life*. Volume 1, *From Pagan Rome to Byzantium*. Translated by ARTHUR GOLDHAMMER. Cambridge: Belknap Press of Harvard University Press. 1987. Pp. ix, 670. \$29.50.

Several attempts have been made in the 1980s to write broad histories of the European family beginning from antiquity. Difficulties arise from the impossibility of acquiring expertise over more than a millennium, and some of the attempts have, understandably, been flawed by fundamental misunderstandings about Roman family practices. To overcome this problem, *The History of Private Life* project, created by Georges Duby and Philippe Ariès, offers a series of chapters written by experts on diverse times and places. In solving one problem, however, the editors have created others. The contributions not only are inevitably uneven but also do not address the same issues and so fail to give the reader a satisfactory idea of the changes and continuities in private life.

The starting point of the series is a long chapter on the Roman empire by Paul Veyne, the volume's editor. Veyne's views on many subjects of Roman social history are regarded by many as idiosyncratic and provocative, and this chapter will enhance that reputation. For instance, his description of relations in the Roman household seems to imply that there was not much difference between the son's and the slave's positions. The head of the household lorded it over both; in many respects, the son was "like a slave" (p. 28), for whom the father's death "signalled the end of a kind of slavery" (p. 29). On the other hand, "the [slave's] master commanded with love" (p. 65) and "punished paternally" (p. 51). One might try to excuse Veyne here for having accepted rather uncritically

the rhetoric of the master class, but even the Romans were aware of the vital distinction between the father-son and the master-slave relationships, whatever their similarities in law. For masters, the relationship was fundamentally exploitative, with the result that the recalcitrant slave had to be driven by the whip, the symbol of servitude. Fathers and sons, by contrast, were bound together by the reciprocal obligations of *pietas*. Slavery was not the benign institution that some southern planters liked to portray—and the Romans knew it.

This chapter is littered with similarly misleading generalizations, some of which run directly contrary to the evidence. For example, no historian familiar with the *lex Iulia de vi privata* could claim flatly that an armed attack by a neighbor “would have been considered a strictly civil offense; it would not have been covered by a penal code” (p. 166). The uninitiated who are unable to identify such misstatements would be well advised to steer clear of this chapter.

Peter Brown’s chapter, “Late Antiquity,” is the high point of the volume, with its analysis of the ideological changes brought on by Christianity, described in Brown’s usual, brilliant phrases. The essay offers a resolution to the debate over the church’s influence on familial and sexual norms. Against the traditional view that Christian teachings revolutionized attitudes toward the body, sexuality, and the family, it has recently been argued that nearly all the new doctrine had been prefigured in the philosophies of the early imperial elite. Brown’s synthesis incorporates elements of truth from both sides. The classical philosophers did teach restraint in sexual activities, but for them it was a “refinement” of the “morality of social distance” in ancient communities: “the upper classes sought to distinguish themselves from their inferiors by a style of culture and moral life whose most resonant message was that it could not be shared” (p. 240). By contrast, the Christians created a community within the empire, one of whose principal defining characteristics was (in one of the many memorable phrases) a “democracy of sexual shame” (p. 243). Whereas for the pagan philosophers sexual restraint was just one of many facets of self-control (gluttony got more attention), for the early Christians “anxious concern for the solidarity of a threatened group” produced a “sharp negative sense of the private . . . Shielded by ‘negative privacy’ from the eyes of men, the heart was held to be totally public to the gaze of God and His angels” (p. 254). This complex and subtle essay must be read by anyone wishing to understand the transition from the classical to the Christian moral systems. But Brown recognizes that the changes in moral pre-

cepts did not drastically alter common practices, about which he has much less to say.

Yvon Thebert’s “Private Life and Domestic Architecture in Roman Africa” provides an interesting description and analysis of the elite’s houses in one region of the empire. As he shows with the aid of numerous floor plans, the enormous houses of the rich served both public and private functions, and the public rooms were not segregated from the private. Sensible and interesting observations are presented about the relationship of domestic architecture to social practices—for instance, in the Christian era “houses became increasingly independent of communal facilities” as their owners put in baths and other conveniences for their private use (p. 392). For all its interest, this chapter is only about very rich North Africans and so has limited value in a general history of private life.

“The Early Middle Ages in the West,” by Michel Rouche, illustrates a general problem encountered in the young subfield of the history of family and household: what level of generalizations should we be aiming at? It will be hard for some historians to take seriously an essay that includes generalizations like the following. “In a society dominated by youth, young men absorbed in enjoyment of the present moment were scarcely troubled by promises they had given. To dominate duration and time is a weak old man’s pretension” (p. 431). “Piety insinuated itself into the gullets of the faithful in a veritable incarnation, a ‘fleshification’ of faith—faith in God and in those to whom he had given power” (p. 447). Some of the demographic assertions are in fact formally meaningless—for instance, “old people were rare, but if a person lived to be 40, his or her chance of survival doubled” (p. 459). Survival until what age? Chance of survival doubled from what?

After Rouche’s essay, Evelyn Patlagean’s final chapter on Byzantium comes as a welcome relief. She is honest with her readers in carefully outlining how the limits of the evidence circumscribe the scope of reasoned generalizations. For the classical historian, perhaps the most interesting part of her account is how monastic institutions came to be an alternative center of private life but one modeled on family roles and sometimes run in the interests of privileged families. In contrast to Rouche, who believes that Christian conversion brought enlightenment and escape from an instinctual animal existence without culture in the West (p. 548), Patlagean recognizes that “enlightenment” was not so easy, as Christian intellectuals in the East continued their interests in “astrology, magic, and alchemy” (p. 636).

In sum, the quality of the contributions is so uneven that one can only hope that the prestige of the series does not lead the unwary to accept what

no critical historian familiar with the evidence would find plausible.

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JERRY WEINBERGER. *Science, Faith, and Politics: Francis Bacon and the Utopian Roots of the Modern Age; A Commentary on Bacon's "Advancement of Learning."* Ithaca: Cornell University Press. 1985. Pp. 342. \$32.50.

In his introduction, Jerry Weinberger characterizes the book under review as an "old fashioned approach to an old treatise." He may well have employed the term "archaic," for what Weinberger has produced closely resembles a good old medieval commentary on a text of an approved authority. The book both benefits and suffers from such an approach. The master chosen is Sir Francis Bacon, who is unabashedly described as the "greatest of the moderns," or as the "founder of the modern age," while the text explicated is the *Advancement of Learning*, first published in 1605. Surprisingly, for a book written in the 1980s, this commentary on a text nearly four hundred years old is defiantly ahistorical. The author singled out Bacon's book for discussion solely because *Advancement of Learning* seemed to him best capable of enlightening us about "the causes and problems of the modern age." Consequently, no effort was made (or seemed necessary) to couch the discussion of the book in terms of early modern English political, social, or cultural history.

As befitting the genre, the commentary is much longer than the text it explicates and mostly takes the form of an encyclopedic compendium of reference to the sources and allusions employed by Bacon. As such, we are given a crash course in classical mythology and literature, which certainly may benefit contemporary readers who are no longer exposed to the heritage of the classical tradition but which does not necessarily add to our understanding of either Bacon or his best-known book. To gain such an understanding, the reader should pay particular attention to Weinberger's own admonition—not to substitute the reading of the original with the commentary.

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JOHN MCCORMICK. *George Santayana: A Biography.* New York: Alfred A. Knopf. 1987. Pp. xv, 612. \$30.00.

George Santayana has had an honored place as a great American philosopher, along with five oth-

ers of the Golden Age (Charles Sanders Peirce, William James, Josiah Royce, A. N. Whitehead and John Dewey). But, along with Whitehead, Santayana has been neglected by philosophers. Whitehead has been kept alive by theologians, Santayana largely by literary scholars. Yet Santayana as a critic of culture has never failed to attract readers, both historically and philosophically minded. A strong revival is underway: a Santayana Society is in its sixth year with a journal, *Overheard in Seville*, and a critical edition is being published by MIT Press. *Persons and Places* continues to attract attention; *The Sense of Beauty* was published last year; *Interpretations of Poetry and Religion* will shortly be available. We are distinctly in a period of Santayana revival, and John McCormick's long-awaited biography is one of several studies.

The pattern used by the author is traditional life and thought. About two-thirds of the thirty-four chapters follow the career, about one-third use titles of his philosophical books. But in the case of Santayana there is constant interaction between life and thought. McCormick comments on the literary response to events and never fails to describe the circumstances of authorship, the influences, and, after publication, the reception, along with reviews and Santayana's later reflections on what he had said. McCormick is well known for studies in contemporary literature. He shows no reluctance to damn the poem *Lucifer* as "dreadful," and he expresses slight praise for nearly all of Santayana's poetry in English: the exception is "The Undergraduate Killed in Battle: Oxford, 1915" (p. 229). McCormick is particularly helpful in analyzing Santayana's relation to other poets, such as T. S. Eliot, Ezra Pound, Robert Lowell, Wallace Stevens. Historians of American culture will be indebted to McCormick's extensive research into the Spanish background and also his investigations of the English, German, French, and Italian stages of Santayana's journey. The political events of war and the rise of fascist dictatorships are not neglected, yet Santayana's detachment from politics is respected. McCormick is also to be commended for handling subtly the complex problems of relations between Christians and Jews.

Santayana remained "remarkably whole in a time of bits, pieces, and particles, utterly honest and serene" (p. xiv). The philosopher is "authentic" in his naturalistic philosophy, which does not exclude spirit, and is presented as satisfying a deep contemporary need of ours.

McCormick follows the best established tradition of interpreting Santayana, that of Columbia's Frederick Woodbridge and his friends in New York, of preferring the early Santayana of the *Life*

of Reason (1905–1906) to the later Santayana of the *Realms of Being* (*Essence, Matter, Truth, and Spirit*). Others may deal with the ontology and metaphysics. McCormick does not ignore the more Catholic and Platonic side, but he chooses to stress the influence of Santayana's freethinking father rather than that of Santayana's sister, Susanna, who would have forsaken the world for a Carmelite convent (a novice who did not make solemn vows, she later married).

What is controversial in the reading of Santayana's career is whether all the hints of male friendships add up to one homosexual act. The evidence is not there, and a reader may be unconvinced, in spite of McCormick's suggestion that such a knowledgeable man simply could not have remained celibate.

What is especially to be remembered from McCormick's biography is his response to Santayana's tragic sense of life. In a letter to James, Santayana feared people would miss in his writing the *tears*: "Sunt lachrimae rerum, et mentem mortalia tangunt!" Not that I moan over the gods of Greece, turned into the law of gravity, or over the stained-glass of cathedrals broken to let in the sunlight and the air. It is not the past that seems to me affecting, entrancing, or pitiful to lose. It is the ideal. It is that vision of perfection that we just catch, or for a moment embody in some work of art, or in some idealised reality . . . And it is my adoration of this real and familiar good, this love often embraced but always elusive, that makes me detest the Absolute and the dragooned myths by which people try to cancel the passing ideal, or to denaturalise it. That's an inhumanity, an impiety, that I can't bear" (letter, Paris, December 6, 1905, quoted on pp. 183–84).

McCormick's biography is true to Santayana's tragic vision.

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DAVID BIALE. *Power and Powerlessness in Jewish History*. Paperback edition. New York: Schocken. 1987. Pp. xi. 244. \$8.95.

The one indisputable fact about the history of modern Jewry is that it has been transformed: by the elimination of the great heartland of Jewry in Eastern Europe, by the rise of the great community in North America, and by the establishment of sovereign government by and for the Jewish people in Israel. How all this, along with many attendant matters, came about is the principal concern of most historians of modern (or contemporary) Jewry; but if there is agreement on many aspects of this vast development, there is much

dispute, too. There is agreement on the factors leading to the quantitative and qualitative transformation of American Jewry in the course of the nineteenth and twentieth centuries. There is less of a consensus on the roots of (as opposed to direct responsibility for) the Holocaust. On the motor forces underlying—or, as some would have it, permitting—the revival of an independent Jewish polity and its broader implications, there is real controversy. The controversy is not only on what it portends. It is also on what it may signify retrospectively. Many historians of Jewry and Judaism continue to look resolutely backward without much regard for what quite recent times may have to teach. There are others (and it is only fair to say that I am of their number) who believe that it is incumbent on the historians of Jewry to take a long, hard look at the full course of the exilic period with such questions as the great contemporary transformation cannot help but raise.

The central issue is power—in all forms and intensities, present and absent, relative and absolute, contingent and intrinsic. Linked to power is the major problem of its significance, both for what might be termed the Jewish ethos, as it has gradually taken shape, and for the Jews themselves—not so much (at all events, not simply) as bearers of a faith, culture, and tradition but as ordinary, mortal men, women, and children.

David Biale, as the title of his short book *Power and Powerlessness in Jewish History* proclaims, is wholly alert to the importance of the issue. Indeed, the principal virtue of the book lies in the very attempt to grapple with it and to lay some of its components bare, and the author is to be congratulated, too, for the boldness of his approach. He sweeps with confidence and an admirable display of learning through the full course of the history of the Jews from antiquity to our own times. He touches (necessarily lightly) on medieval Jewry, on the Jews of the Enlightenment (first the Enlightenment of the Gentiles, then their own), on the irreversible physical and moral crushing of European Jewry in modern times, on resurgent Israel, and, finally—and it is telling that he concludes his argument here—on American Jewry and on what the author terms "contemporary Diaspora power." Biale's central thesis, broadly stated, is that, contrary to common belief, the Jews were rarely truly powerless. One way or another, from time to time, they wielded considerable power to shape both their own affairs and those of others, whether as a collectivity or through identifiable representatives. And if they did not have this capacity *de jure*, they certainly did (and do) have it *de facto*.

It must be said, of course, that the long, complex history of the various Jewish communities is not

amenable to any flat judgment. By the same token, how far the communities need to be studied as parts of a whole and how far as discrete entities are questions of the first order of historiographical importance in themselves. In any event, the history of the Jewish people will bear a very remarkable number of distinct and even contradictory interpretations. So much must always be admitted. Yet there is something excessively contrived, if not perverse, about an argument designed, as it seems, to refute the common view—an ordinary observer's view long antedating the horrors to which twentieth-century Jewry has been subjected but none the poorer for that—namely, that, in any reasonable, final analysis it has been the *weakness* of Jewry that has been decisive for its fate and, quite possibly, for salient features of its internal development as well. So, if this is a useful and in some ways provocative book, certainly one that deserves to be read, its thesis is substantially less than persuasive.

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NICHOLAS CANNY and ANTHONY PAGDEN, editors. *Colonial Identity in the Atlantic World, 1500–1800*. Princeton: Princeton University Press. 1987. Pp. xi, 290. \$35.00.

These invariably interesting, generally well-written, and enormously perceptive essays cover a range of cases in which the contributors have kept both local and comparative considerations of colonization in mind. Although the volume is not arranged chronologically, the frequent cross-referencing combined with a magnificent introduction and afterword provide the collection with a welcome cohesion and order. As John Elliott points out in his superb introductory essay, the Atlantic world of European conquest, commerce, colonization, and control had much in common irrespective of the system of empire and the timing of imperial integration. The English settler's experience in Ireland—a relatively short hop across the channel—did not vary greatly from the English settler's experience in the Americas—a long and difficult journey across a mighty ocean. At the same time, the English settler's experience in the Americas did not vary greatly from that of the Spanish, Portuguese, French, or any of the other Europeans. "A Spanish hidalgo in Mexico and an English gentleman in Virginia may well have had more in common with each other than with merchant and artisan settlers from their own native countries when it came to envisaging the

form of community they wished to establish," says Elliott (p. 6).

In his examination of the emergence of the Anglo-Irish communities in Ireland, Nicholas Canny argues that after 1541—when Henry VIII declared himself king of Ireland rather than, as had been the custom, lord of Ireland—the English regarded Ireland as a colony. The attitudes of the new English to the land, to the native people, and to those who were previously settled in Ireland were common imperialist attitudes. "For the Spaniards, the French, the English, and the Portuguese in America, no less than for the English in Ireland," suggests Canny, "the need to transform the indigenous inhabitants of the land they occupied into civil people was held to be crucial to any attempt to mold a new civil society. Conquest, as the conquerors saw it, involved eventual acculturation" (p. 175). The conflicting identities evident in Ireland also arose in Canada, though split between French, English, and Spanish elite subgroups. Gilles Paquet and Jean-Pierre Wallot insist that institutional fragmentation inhibited the growth of a Canadian identity similar to the one that manifested itself in Ireland or elsewhere in the Americas.

Jack P. Greene, writing on Barbados, and Michael Zuckerman, writing on British North America, both illustrate the varying nature of British colonial experience. Greene succinctly lays out the process by which British settlers arrived at their identity of being Americans: "the process of identity formation seems to have involved three sequential, if not always sharply distinguishable, phases. During the first phase characteristics of place usually assumed primacy. That is, settlers and their sponsors tended to identify their society in terms of the nature and potentialities of the place in which they lived. During a second phase, they tend to define themselves more in terms of how they were organizing their social and cultural landscapes and the extent to which those landscapes did—or did not—conform to inherited notions and standards of how such landscapes should be organized. Finally, during a third phase, they gave increasing emphasis to their predominant characteristics as people and to the common experiences shared by themselves and their ancestors" (p. 217). Unlike Ireland, Barbados had no native inhabitants to be civilized, and so within a few years of its settlement the main attraction rested on its economic potential. By 1650 little was made of re-creating English society, a situation accentuated by the presence of growing numbers of servants and slaves. Zuckerman argues that the British colonists "always strove to be Britons and that they attained political independence at the end of the eighteenth century without ever declar-

ing their common character or distinctive identity" (p. 115).

In Brazil the process of identity did not follow the convenient sequence suggested by Greene. Stuart Schwartz claims that "the formation of colonial identity in Brazil was infused with ambiguities" (p. 39) and that hand in hand with the colonists' love for their newly settled land went a strong bond with the Portuguese crown. But from the beginning the multi-ethnic and multiracial Portuguese colonial Brazil was quite distinct from Portugal. The colonial elites, bolstered by their economic power vis-à-vis the metropolis, aspired to be different and better than their Iberian cousins, but the transfer of the royal court to Rio de Janeiro merely accentuated the differences between metropolitan and colonial factions, while the fear of servile revolts increased the dependence of the colonials on the metropolitan army.

Spanish America presented a variety of situations. Generational differences, the presence of large, politically complex—though subordinated—native populations, and mining and agricultural wealth (compounded by shifts in official attitudes) produced varying local responses on the part of the colonists both to their environment and to their sense of themselves. As in the case of Brazil, self-definition in the Spanish Americas did not merely comprise a distinction between the ethnics of metropolis and colony. Rather, the definition of self involved a searching rationalization of the legal, social, and political position of the criollos within the panorama of Indians, mestizos, mulattos, criollos, and *gachupines*. By the eighteenth century this process involved an implicit denial of the Americas as the patrimonial estate of the monarchs of Castille as well as a reworking of the myths of Indian "historical identity" (p. 68). In New Spain the evaluation and celebration of the Virgin of Guadalupe played an important symbolic role in creating viable links across class, race, and time.

The authors skirt politics and mostly fail to pursue the implications of race, color, and class, but together they show that a general set of circumstances catalyzed the development of these regional identities and that such a development was neither uniform nor necessary to political independence. Nevertheless, as Anthony Pagden and Canny state in their conclusion, political independence could not have been possible without a prior consciousness of self. For this reason the book provides a substantial contribution to our understanding of the rise of nationalism in Ireland and the American world.

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GIANNI VAGGI. *The Economics of François Quesnay*. Durham, N.C.: Duke University Press. 1987. Pp. xv, 247. \$37.50.

This is a book by an economist written primarily for other economists. It is filled with mathematical formulas and phrases like "value magnitude." Nonetheless, it does offer much that will interest Old Regime historians in general and historians of economic thought in particular.

As his central thesis, Gianni Vaggi proposes that Quesnay and the other physiocrats established the basic analytical structure of classical economics. Quesnay's examination of prices, market forces, and the process of production was more sophisticated than many of his critics have recognized. One reason for scholars generally failing to perceive the full richness of Quesnay's thought has been their assumption that the *Tableau économique* represents a synopsis of his essential ideas. Instead, as this book demonstrates, one should realize that Quesnay declined to compose a *summa*. Therefore, ample consideration must be given to all of his writings.

Vaggi contends that Quesnay's most important contribution was his systematic investigation of the creation of surplus. A surplus permits a society to reinvest capital and generate continued growth. Quesnay's error was his belief that only agriculture could produce a surplus; hence his assertion that trade and industry were "sterile." But his focus on the question of surplus became important as the starting point for Adam Smith, David Ricardo, Karl Marx, and others.

Throughout the book Vaggi offers numerous thoughtful commentaries on secondary topics. He notes, for example, that the physiocrats were not fully committed to the doctrine of laissez faire. They adopted the notion of free trade only as a last resort, so that French agricultural products could have easy access to the international market. Vaggi also observes that the physiocrats were much like the mercantilists in one respect: both groups sought a favorable balance of trade for France and assumed that if one country benefited from international commerce then others must lose.

Although Vaggi organizes his material in original fashion, he often exaggerates the novelty of his arguments. For instance, he points out that Quesnay tried unsuccessfully to reconcile the emerging capitalist mode of production with the existing seigneurial system. Although Vaggi cites Elizabeth Fox-Genovese, he does not fully acknowledge that she has already examined this topic extensively in her book on the origins of physiocracy.

Moreover, the book exists in a historical vacuum. Rarely does Vaggi confront the economic

realities that existed alongside the economic theories of Quesnay and his colleagues. Vaggi correctly reports Quesnay's fears about a crumbling economy: among other things, Quesnay maintained that France's population had declined from twenty-four million in 1650 to sixteen million in 1750. Yet Vaggi does not tell his reader that such estimates as this were highly inaccurate. In the few cases in which Vaggi does describe actual conditions, he relies heavily on outdated studies. His tenuous grasp of economic realities is revealed by his surprising statements about the emergence of an industrial bourgeoisie and the establishment of many medium-sized factories throughout the country by the middle of the eighteenth century.

THOMAS J. SCHAEFER
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ANTOIN E. MURPHY. *Richard Cantillon: Entrepreneur and Economist*. New York: Clarendon Press of Oxford University Press. 1986. Pp. vii, 336. \$48.00.

Richard Cantillon's claim to historical attention rests on his seminal treatise, *Essai sur la nature du commerce en général*, published in 1755, twenty-one years after his death. An Irish Jacobite, he was born into a landed family in County Kerry sometime in the 1680s and emigrated to France sometime between 1690 and 1708, when he applied for French nationality. Cantillon rose rapidly in financial circles in part on his uncommon talent, in part on his close connections with an influential circle of emigre Jacobite bankers, notably, his uncle the chevalier Richard Cantillon and Sir Daniel Arthur. By 1718, he was well established as a banker in Paris, and by 1720 he was advising John Law, from whose financial ventures he profited. Cantillon's successful operations during the South Sea Bubble and Mississippi System earned him vast wealth, impressive social connections, and the passionate enmity of less fortunate speculators, in particular, Joseph Gage and Lady Mary Herbert. Subsequent court battles dominated the final years of his life. He purportedly died in 1734 in a fire in his London house in Albemarle Street.

With a rare combination of devotion and dogged determination, Antoin E. Murphy has unearthed every surviving detail about his hero's life and, from far-flung fragments, constructed the most thorough biography we are likely to get. Against all the odds of sparse and disparate evidence, he has written an engrossing if frustrating account. For the sketchy evidence has forced Murphy to piece together clues rather than re-create a life. Cantillon emerges from the traces of his presence and activities, not from direct accounts

of his thoughts and feelings: his personality ultimately remains elusive. Withal, Murphy admirably locates the shadowy Cantillon in the world of early eighteenth-century international finance and, more precariously, of letters. And his denouement, set in the yet more shadowy underworld of prisons and international adventure, evokes the spirit of Joseph Conrad. Cantillon, it seems, might not have died in his sleep in the fire; he might have been murdered, or he might not have died in 1734. Six months after the fire, a certain "Chevalier de Louvigny" mysteriously appeared in Surinam and, in his flight from the local authorities, left behind a large collection of documents pertaining to Richard Cantillon.

Murphy fleshes out the skeletal account of Cantillon's life with a wealth of observations on the interlocking, international world of early eighteenth-century banking and commerce. But the central justification for a biography of Cantillon inescapably remains the *Essai*, which no less a figure than Joseph Schumpeter credited with inaugurating the modern study of the economy. Murphy devotes a chapter to the *Essai* and forcefully underscores its originality and significance. But in pressing Cantillon's claims as the premier modern economist—foreshadowing the physiocrats and Adam Smith—Murphy misses the greatest significance of his work. For, if Cantillon grasped the circular flow of economic life, he did not match the physiocrats' or Smith's intuitive understanding of capitalist production. Cantillon's concerns remained, as Murphy knows, the relation between a landed society and burgeoning commerce and the relation between an increased velocity of monetary circulation and wealth. These concerns, for all the analytic sophistication with which Cantillon pursued them, reflected the world of merchant capital in which he was so astute a participant, but they did not encompass those capitalist relations that transfixed his successors.

By recovering the elements of Cantillon's life and times, Murphy has made a significant contribution. If, in his enthusiasm, he has sometimes exaggerated Cantillon's importance and influence, he has compensated by linking the world of international finance to the more familiar worlds of high society and letters. Murphy might, nonetheless, have displayed greater generosity in acknowledging his own predecessors. Among others slighted in the book, Jacob Price has masterfully charted the links of international finance, and John Boshier has laid the claims for Vincent de Gournay's seminal role in the development of French economic policy.

ELIZABETH FOX-GENOVESE
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HERMAN VAN DER WEE. *Prosperity and Upheaval: The World Economy, 1945–1980*. (History of the World Economy in the Twentieth Century, number 6.) Translated by ROBIN HOGG and MAX R. HALL. Berkeley and Los Angeles: University of California Press. 1986. Pp. 621. \$32.50.

GEROLD AMBROSIUS and WILLIAM H. HUBBARD. *Sozial- und Wirtschaftsgeschichte Europas im 20. Jahrhundert*. Munich: C. H. Beck. 1986. Pp. 329.

Herman van der Wee's book is devoted to the two major trends of world economic development that have appeared since the Second World War. The first part of the book deals with economic growth under the heading of successes and problems. In the second part the well-known Belgian professor elaborates on the world-wide economic institutional framework. Van der Wee expertly combines his extensive historical knowledge—his books and articles cover a variety of problems from the Middle Ages or early modern period to the story of Belgian banks during the twentieth century—with his strong commitment to economic theory. He is able to refrain from being too abstract. And, at the same time, he is never too simple, nor does he simply narrate. His presentation covers a success story: after tremendous war losses, a quick recovery. But it was a recovery entirely different from that of the 1920s, when an attempt was made to return to the good old times, to a liberal economy with no state intervention, to basically a market economy without any attempt to apply new economic theories, a policy that finally resulted in the Great Depression. The silver fifties as an introduction to the golden sixties carried out entirely different premises. The Keynesian revolution was flourishing; cycles, slumps, depression became obsolete notions belonging to the past; economic growth became the word of the day. Van der Wee does not overlook the question of the managerial revolution; in his analysis one can find the proper place for Joseph Schumpeter whose innovation theory became one of the standard explanations of the golden age. None of the important economic theories escapes van der Wee's profound analysis: the Malthusian view of population trends, the report of the Club of Rome concerning the exhaustion of raw materials, Denison's theory ascribing a large part of the economic growth to education and schooling, the Lewis model with its unlimited labor supply.

Abundant statistical material—from research investment to economic performance, from technical interventions to wages, from income growth to income distribution—supports van der Wee's theories. The imperatives of demand lead his

explanation to foreign trade where he strongly supports the neoclassical hypothesis of the Heckscher-Ohlin theory.

In the second part of the book, one can feel the strength of the author's historical understanding; even when he analyzes the theoretical starting point of the mixed economy, he is able to demonstrate his wide reading in the ideas presented during the last half-century concerning private and public enterprises, mixed economy, government planning and its limits. An institutional framework cannot be traced back just to independent countries. Recent development brought the world closer together and gave world-wide attention to a new economic world order. The Pax Americana that emerged after the Second World War, with the vision of an absolute free-trade economy and the establishment of a monetary system in which the dollar dominated without any serious challenge, slowly faded away, yielding more and more space to a world system in which features of new mercantilism and constant troubles with floating foreign exchanges appeared. The end of the Bretton Woods system is presented with careful historical and economic arguments and shows to what extent van der Wee is able, intellectually, to control his field.

Is van der Wee's account a success story? Yes, partly it is. But recent years have given the author enough warning. His historical mentality both excludes and is too much impressed by successes. He knows to put success in historical perspective, where the eternal race never stops, where successes cannot be maintained forever, where failures will never disappear, and where the ever-emerging new challenges have to be met constantly with new ideas, new innovations, new systems, where continuity can be maintained merely by changes.

Critical remarks certainly can be made about van der Wee's book. Controversial judgments concerning the post-World War II recovery can be discussed. For instance, Alan Milward, in his recently published book, disagrees with van der Wee's view on a number of points, and those points certainly deserve further discussions. The subtitle of this book, "The World Economy," and the space devoted to the Eastern bloc are not nearly enough for any meaningful discussion. In this short review, however, I do not feel it necessary to point out further shortcomings, because the book is worth reading and learning from.

Van der Wee's work is a profound analysis of a quarter of a century of economic history; Gerold Ambrosius and William H. Hubbard attempt to give a concise survey of the social and economic history of the twentieth century in Europe. They have a couple of strong points: ample statistical

material is included, and the bibliography of the book provides good evidence that the authors are quite familiar with the literature. Seemingly they are more interested in social history, social and demographic changes, than in economics. Even when discussing economic history, they are more interested in the distribution of social products and in some economic policy issues than they are in factors and processes in economic growth. Their various regional models of economic growth are intelligent but too general. For someone who wants to get a short introduction to European economic history, the book can certainly be useful as an introduction.

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JOHN A. GARRATY. *The Great Depression: An Inquiry into the Causes, Course, and Consequences of the Worldwide Depression of the Nineteen-Thirties, as Seen by Contemporaries and in the Light of History*. Paperback edition. Garden City, N.Y.: Anchor Press of Doubleday. 1987. Pp. x, 292. \$9.95.

John A. Garraty, as his subtitle indicates, has read widely in what contemporaries had to say about the Great Depression and what historians and social scientists have had to say since. He has also digested and organized well what he has learned, and in the book under review he offers both an engaging and well-written account of this and a series of provocative generalizations about the Depression experience and its place in history. The work is intended mostly for a general audience, not specialists in economic, social, or public policy history. But its international dimension, a rarity in depression studies, is something from which specialists can profit as well.

In reconstructing and interpreting the Depression, Garraty begins with chapters on the theories of causation and the developments taking place through 1932. He then devotes three chapters to "what it did" (to farmers, workers, and the jobless), moves next to four chapters on its intellectual and political impact, and concludes with a chapter on "how it ended" and the legacy it left. Along the way, he provides a wealth of interesting detail about various episodes, processes, and individuals. But also emerging are several major interpretive themes. One is the perpetuation of the Depression by narrow nationalistic outlooks and stubborn efforts to apply conventional wisdom. Another is the failure of radicalism and the concomitant resiliency of capitalist arrangements. A third is that the Depression was only a "milestone" in the coming of managerial and welfare states, not the

creator of them or even a "major turning point" (p. 249) in their historical development. And a fourth is that political responses, despite national peculiarities and worldwide similarities, were of two general types, one exemplified by Adolf Hitler's New Order and Franklin D. Roosevelt's New Deal, the other by policies in Britain and France.

The last of these themes, familiar to those who have read Garraty's 1973 article in the *AHR*, is likely to be the most controversial. It rests on his showing of striking similarities between the Hitler and Roosevelt anti-depression programs, similarities having to do particularly with the role of charismatic personal leadership, the mobilization of popular psychic energies, the primacy of politics, and the kinds of political appeals used and administrative interventions undertaken. These, Garraty argues, do not mean that the New Deal had "a large fascist element" (p. 209). But, in his view, they do justify seeing the German and American programs as variants of a larger pattern of reaction distinct from that in other industrial nations. The latter, as exemplified by reactions in Britain and France, lacked the same kind of psychic intensity, national spirit, folkish appeal, and investiture of power and hope in a national leader.

Garraty's judgments about remedies for the Depression reflect a basic acceptance of Keynesian economic postulates. Deficit financing was the remedy that worked. Corporatism was "essentially archaic" (p. 191), not a prelude to the planning required by modern capitalism. Trade controls only made matters worse, and monetarism rested on faulty assumptions. Yet, in places, the analysis does concede important points to critics of this new conventional wisdom. It recognizes, for example, the importance of business confidence and private power, the positive side of "import substitution," and the possibility that the new wisdom may be as wrong for today as the old was for the 1930s.

The book's flaws are minimal. Some arguments need further development, and there are a few factual errors. The President's Reemployment Agreement, for example, is given provisions it did not have (p. 192), and the Fair Labor Standards Act was passed in 1938, not 1940 (p. 196). But, on balance, the book succeeds marvelously well in achieving the author's intended purposes.

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RONALD E. SEAVOY. *Famine in Peasant Societies*. Westport, Conn.: Greenwood. 1986. Pp. xii, 478.

What does one do with an author who claims to have discovered a Great Truth? One answer is to

add the claim to the pile of academic junk that litters our lives. But there is an idea somewhere here to be rescued, and it is worth digging to find it.

Peasant societies, Ronald E. Seavoy tells us, suffer periodic shortages of food "regardless of climate, soil fertility, population density, crops grown, or cultivation techniques used." This predicament follows from his definition of peasants as "cultivators who produce subsistence quantities of food" (p. 10). Subsistence is defined as "the attempt by peasants to grow sufficient food for their own consumption with the least expenditure of labor on the assumption that all crop years will be normal" (p. 10). Peasants believe in "equalized" access to the village land, equalized sharing of harvests, and "minimal labor expenditures on food production" (p. 387). These attitudes account for their difficulties.

Seavoy's Great Truth is that the solution to the peasant problem is "revolutionary political action" and not economic policies, because "economists have failed to accurately describe and analyze the food producing capabilities of peasant societies" (p. 353). According to Seavoy, "99 percent" of economists are guilty of this failure, among them T. W. Schultz, R. H. Tawney (who would be surprised to find himself in this company), J. L. Hammond and Barbara Hammond (who would be equally surprised), Ragnar Nurske, God (see, according to Seavoy, *Holy Bible*, Genesis, chap. 3: 17–19), Hla Myint, Joel Mokyr, J. K. Galbraith, J. C. H. Fei and Gustav Ranis. Only Thomas Malthus got it right; "Malthus is as accurate today as he was in 1800" (p. 369). One wonders whether to admire him for his prescience or to be sorry for him for failing to make the 99 percent club. Although economists sometimes apply neoclassical economics uncritically to peasant cultures, Seavoy's assertions ignore or distort a great deal that is good in development economics.

Seavoy buttresses his argument by four historical studies. Much of what he says about England and Ireland is wrong. England never was a peasant society, and it is incorrect to equate the common fields with subsistence farming and enclosure with commercial farming. It is untrue that from 1540 central government pursued a conscious policy to commercialize agriculture. The treatment of Ireland is even crasser. The description of the Gaelic land system and Gaelic attitudes is a travesty of reality. The statement that, as a result of the colonization of Ulster, "most of the remaining Irish peasants . . . [were] resettled in marginal lands" suggests that Seavoy understands neither the process of settlement nor the difference between proprietors and peasants. According to Seavoy, peasants switched from cultivating oats to

potatoes because potatoes required less effort. I wonder whether he has ever grown potatoes. Discussion throughout is confused by a naive view of Irish political history. More fundamentally, the evidence does not support the central thesis that Ireland was forever teetering on the brink of subsistence.

What are the volume's good points? I enjoyed the two studies of India and Indonesia, although I wonder what an expert would think. There is a mass of useful factual information and a first-rate bibliography. Finally, there is Seavoy's ability to irritate. A provocative scholar is worth a dozen who soothe with soft soap, even though his Great Truth is as insubstantial as the bubble reputation.

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CAROLYN MARVIN. *When Old Technologies Were New: Thinking about Electric Communication in the Late Nineteenth Century*. New York: Oxford University Press. 1988. Pp. 269. \$34.50.

Our understanding of the technological past begins with the artifacts—the steam engines, old radios, and historic buildings that are preserved for the benefit of future generations and the historians of technology. No period in American history has produced a richer harvest of artifacts than the last quarter of the nineteenth century, when the telegraph, telephone, motion picture, and wireless began to transform life in the industrialized West. It is not surprising, then, that the history of communications of this period is strewn with laboratory equipment and the happy progression of one improved machine after another. But, as Carolyn Marvin points out, beginning the study of the twentieth-century mass media from the perspective of the machine obscures some important social issues raised in the awful advance of the electronic media. She has chosen to examine the process of social adjustment around these new machines at the point of their introduction, arguing that "every essential concern about the promises and risks of broadcasting as a communicative form" had been articulated before the turn of the century (p. 233). Her book sets out to show the ensuing debate, especially the conflicts and fears thrown up in discussions about the possibilities of instant communication. In describing the arena in which complex social issues were negotiated, she defines interest groups and charts their struggle to defend the social boundaries against machines that permitted outsiders to cross existing lines of race, class, and gender.

The book draws heavily on the electrical and

popular press of the time, a convenient source of evidence about the technical elite of communications but by no means a reflection of the general debate about new technology. The view from government, the attitude of business, and the random, but no less important, impressions from literature are not surveyed. The narrowness of the source material is the major failing of the book, for the author has to labor to produce signs of a dialogue about the social uses of new communications. There certainly was conflict within the ranks of electrical practitioners and users at this time, but when it came—as in the “battle of the systems” over the introduction of high voltage supply networks—it was centered on the more immediate concerns of economy and safety. There is no lack of material about the social consequences of disruptive ideas such as the coin-slot telephone; what is missing in this book is evidence that its introduction caused any debate. The important decisions appear to have been made in the anonymity of corporate board rooms rather than in any public forum.

The paucity of evidence sometimes leads Marvin to build great edifices on small foundations, reading volumes of meaning into minor actions. The most important interest group she describes comprised electricians who formed a technical elite determined to extend their control over new communications systems. Yet to interpret their practical jokes as part of their defense of technical leadership and the social class based on it is going too far. The jokes about outsiders, the pranks played on one other, were a common experience of work in the nineteenth century; they were more a reflection of the craft tradition of freedom in the workplace than the expression of a new technological order.

This book is best summed up in its subtitle: *Thinking about Electric Communication in the Late Nineteenth Century*. Although Marvin has not succeeded in showing a dialogue about the uses of new technology, she has described the variety and complexity of the response to the possibilities of modern communications. She has charted a course into unknown waters; the originality of her analysis and thoughtful questions provide a valuable perspective on this critically important period in the history of American technology. Artifacts of history are still alive and well, but there are some attractive alternatives on the horizon.

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J. ROGERS HOLLINGSWORTH. *A Political Economy of Medicine: Great Britain and the United States*. Balti-

more: Johns Hopkins University Press. 1986. Pp. xix, 312. \$37.50.

This work is addressed to health policy analysts rather than to historians. As a comparative study of the organization of medical care in Britain and the United States, it draws heavily on secondary sources in the history of medicine and on the work of such historians as Brian Abel-Smith, Charles Rosenberg, Paul Starr, and Rosemary Stevens. Although J. Rogers Hollingsworth's intent was to synthesize a considerable body of historical scholarship, the whole is less than the sum of its parts: generalizations are asserted with more conviction than evidence, and much of the argument lacks a coherent theoretical structure. At times, the author seems to reach for a class analysis; at others, he speaks in marketing terms of “consumers” and “providers” and of the relative “homogenization of demand” said to characterize the British system (p. 67). Only at the end of the book, in a final chapter called “Theoretical Perspective,” do we discover that this seeming confusion is deliberate, as Hollingsworth writes that he has endeavored to make his discussion equally “meaningful” to “neo-Marxists” and to “neo-Weberians” by mixing the terms common to each. Given this odd ambition, he may probably be congratulated for making as much sense as he does of the argument that “consumers” wielded relatively more power in Britain than in the United States and thus achieved a more equitable distribution of medical resources.

The central sections of the book, in which Hollingsworth compares the British and American systems on the criteria of technological innovation, equality (of access and distribution), and cost containment, are the most interesting and provocative. Most readers will not be surprised to learn that the British system is much more effective in providing equity of access to medical care and also in controlling costs: the National Health Service, which provides free medical and hospital care to most of the British population, is also one of the least expensive systems among advanced industrial countries. It is interesting to discover that inexpensive, highly efficacious technologies (such as vaccines) are diffused at approximately the same rate in both the British and American systems, while the United States is much more receptive to the diffusion of “expensive technologies that are financially beneficial to providers” (p. 177). There are, for example, 46 percent more CAT (Computerized Axial Tomography) scanners in Los Angeles than in the whole of the United Kingdom, and more CAT scanners in California than in all of Europe (p. 179). While the United States has more high technology medicine

and more surgery (coronary artery surgery is nine times more frequent in the United States than in Britain), these differences may not have a measurable impact on health status. At least in terms of crude indicators of health status, such as life expectancy at birth and infant mortality, Great Britain does better than the United States.

Hollingsworth's book will be a useful source of information for those arguing the relative benefits of the British and American systems of health care. In his epilogue, however, he questions whether medical care is adequately addressing the main health needs of either country: he notes that neither Britain nor the United States pays much attention to preventive medicine, health education, environmental or occupational health, and that neither deals well with chronic illness, including chronic mental illness, and the ills of the elderly. These points are made only in passing, but they are essential considerations for any future reforms of the health care system.

Hollingsworth argues the advantages of centralization in the financing and planning of medical care and compares the British system favorably with the American one. Although his sweeping generalizations, on the basis of two examples, are often questionable, the more detailed comparisons of medical care organization are instructive. Historians who find his historical generalizations lacking may nevertheless be interested in the social policy questions provoked by the comparisons of these two health systems.

ELIZABETH FEE
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ARCHER JONES. *The Art of War in the Western World*. Urbana: University of Illinois Press. 1987. Pp. xix, 740. \$34.95.

Like most terms that combine an intellectual concept with professional concerns, the "new military history" has a spurious clarity. It indicates a broader approach to the study of war than that of conventional campaign histories or derivative accounts that do not probe beneath the glamorous and brutal surface of the subject. But the content of the new military history varies. It is often identified with research shaped by sociology, anthropology, and psychology. It may well include the study of military institutions on the basis of archival sources and comparative analysis, or biography that combines narrative history with the history of ideas and politics, or syntheses that stress the interdependence of all lethal and non-lethal elements in war and their interaction with other aspects of their environment from social mobility to ethics. The newness of these ap-

proaches is questionable. Eighty years ago such pioneers as Hans Delbrück, Otto Hintze, and Jean Colin took as comprehensive a view of war as anyone does today, even if our assumptions and methodologies may differ. But "new" is also meant to signify the growing interest in a field of study that until recently counted for little in the American historical profession. In this sense, in particular, the word is misplaced. The new military history is not so innovative that it compels acceptance; rather, our profession itself has become more receptive, in part because some other subjects and methodologies have lost a measure of their appeal, in part because public attitudes have shifted. But however misleading the term, for conventional historians of war the new military history constitutes both a boon and a threat. They appreciate the changed atmosphere but worry that their subject may disappear as the history of war expands to comprise more than the clash of armed men.

Archer Jones defines his book by contrasting it with the new military history and offers it as a corrective. His purpose is to "trace and explain . . . the changes in certain operational variables over most of the span of Western warfare for which we have a record" (p. xvii). This means "omitting all noncognitive aspects" of war "and leaving to others virtually all of the affective domain of warfare, such as morale and motivation." The book "ignores not only most political factors but also the new military history of the environment of armed forces. It defines strategy narrowly, deleting most grand strategy and the integration of political, economic, military, and naval ends and means" (p. xvii). Not that the author believes it is unimportant to study the areas he excludes—which are so numerous that the title of the book must be considered a misnomer—but he holds that "today operational history seems to suffer from neglect, and I hope that this work . . . is an assertion that the new military history should also revive and improve analytical operational history" (p. xvii).

The book, in short, is meant to be a history of tactics—how soldiers fought—and of certain logistical and operational elements—how soldiers were equipped, maintained, brought to combat, and maneuvered before and during battle. It begins with a section on the Greeks and Romans, which, however, expands on the program just outlined by numerous references to politics and society and by positing two basic strategies, labeled "raiding" and "persisting," that inevitably are shaped by political motives. The work continues with sections on medieval and Renaissance warfare and proceeds century by century to the recent past. A lengthy conclusion recapitulates the gist of the earlier sections and offers a taxonomy of strategy, which

demonstrates that, whatever the terminology, even the most determined operational historian cannot delete political and social elements from an analysis of the ways armed forces fight.

Jones is frank to state that his "account of Western warfare, which makes no pretense at originality, came from the standard secondary works" (p. xviii). A precise history of tactics, logistics, and operational methods over the past twenty-five hundred years might nevertheless have its uses. Unfortunately, the author has not compressed his wide reading into a systematic and intellectually defensible presentation. Themes that his programmatic introduction excludes keep appearing; his "narrow" definition of strategy proves untenable; contradictions and inconsistencies abound. A few examples drawn from the opening section are characteristic of the whole. In his discussion of the Greek phalanx Jones asserts that "only the front rank fought" (p. 2). The next page acknowledges that there are two schools of thought on the matter, and the author seems to agree with both. An observation that "the small city-states of Greece normally fought wars for limited objectives" (p. 2)—true in earlier times, less so in the fourth century—becomes the basis for concluding that this "made it easy for them to admit defeat"—an orphic statement that takes us far beyond the realm of tactics. Yet, although one might expect a history of tactics to pay particular attention to numbers, not even estimates of opposing strengths are provided in accounts of the battles of Marathon and Plataea. Throughout the volume the descriptions of battles, included to illustrate various tactical systems, are so fragmentary as to be largely useless—a particularly surprising weakness in this kind of work. Alexander the Great's effort to end resistance to his occupation of Persia by slaughtering civilians prompts the comment, "Terror can be an effective weapon only if its victims, as Clausewitz points out, believe that the terrorism can continue indefinitely" (p. 63)—an astonishing pronouncement had Clausewitz made it. It turns out that the author has paraphrased not a reference to terrorism but a part of Clausewitz's thesis of the dynamic of war between states that suggests that a belligerent may be coerced into accepting unfavorable terms if the alternative is even less pleasant, but that alternative "must not of course be merely transient—at least not in appearance" (*On War*, book I, chapter 1). Jones's only other reference to Clausewitz is equally inaccurate and misapplied.

Tactics, logistics, and strategy are suitable subjects for the old and for the new military history. There is a place for even the most specialized technical approach to these components of war, although the greatest advances in our understand-

ing of the ways men actually fought have come from historians who looked beyond military technology and the parade ground. What does seem unnecessary is to restate in a confusing, haphazard manner the findings of past research. Readers who wish a more insightful introduction to the material discussed by Jones might with profit turn to Delbrück's classic work or to such recent syntheses as Hew Strachan's *European Armies and the Conduct of War*.

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JAMES TURNER JOHNSON. *The Quest for Peace: Three Moral Traditions in Western Cultural History*. Princeton: Princeton University Press. 1987. Pp. xx, 300. \$30.00.

This study has a number of useful aspects, unfortunately undermined by many serious defects.

Its main usefulness lies in the frame of reference James Turner Johnson proposes for analyzing the quest for peace in "Western culture," not because this frame of reference is adequate to the purpose but because it raises interesting questions. Johnson distinguishes three main traditions in the history of this quest: first, what he calls "radical Christian sectarianism" (later "absolute pacifism"); second, "humanist utopianism" ("internationalism," "perpetual peace" theories, or "world order" theories); and third, the "just war" tradition. Johnson seeks to identify distinguishing features of the three traditions, their continuities and discontinuities over time (from early Christianity to the present), some of their underlying assumptions and contradictions, and what he sees as their (rather dim) prospects for coalition. There is much food for thought and debate here.

But the book is sadly marred by Johnson's twofold hidden agenda: first, to defend the Roman Catholic Church of the late classical and medieval era against charges of having betrayed the early Christian commitment to pacifism; second, to legitimize just war theory as one of the three "major forms this quest [for peace] has taken" in "Western" culture (p. 279).

Although some readers may reject the argument Johnson presents for just war theory or question the space and weight it is given throughout the book as a foil against which to assess the other traditions, this is actually one of the most useful features of the work, since it offers a perspective not often presented in the history of peace traditions, and one deserving thoughtful attention. However, thoughtful attention is deflected from this worthy purpose by Johnson's

extended polemics against earlier scholars of the history of pacifism. These arguments, rife with false dichotomizations and inconsistencies, prove ponderous and unpersuasive in the end.

Johnson does not claim to offer a comprehensive history, but his perspective on what constitutes pacifism or a "peace tradition" is so narrow that many readers will find fault with the exclusions, omissions, and, in some cases, misrepresentations in the peace traditions he defines. Some will be offended by the ethnocentrism he betrays, both in defining pacifism itself as a "Western" phenomenon (p. 174) and excluding all non-Western traditions from consideration, and in the failure to recognize that there are other *western* peace traditions, such as those of Native Americans. Johnson will be faulted, too, for the almost total omission of women in the history of the quest for peace (several passing references are made to Christine de Pisan, not noted in the index); for total omission of socialist antimilitarism (even from just war theory); and for the cursory and contemptuous treatment he accords to the history and theory of nonviolence (pp. 248–53).

These faults are less easy to forgive since Johnson adds little information to what is available in other studies of the history of peace traditions, and the informed reader may be sorely tempted to toss the book aside in sheer irritation. Those who persist, however, may find that, as an interpretive study with a particular polemical perspective, it does offer some significant rewards.

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VINCENT GEOGHEGAN. *Utopianism and Marxism*. New York: Methuen. 1987. Pp. 164. Cloth \$37.50, paper \$10.95.

This is an attractive short discussion of a theme that, at least until recently, has seemed perennially important. Vincent Geoghegan understands that utopianism is not merely an intellectual exercise practiced by a few imaginative writers but also an expression of the deep human need to imagine something beyond what exists and, in so doing, to construct what Ernst Bloch called a "principle of hope." Following the path-breaking discussions of Walter Benjamin and Theodor Adorno (though citing only their student, Hans Enzenberger), Geoghegan recognizes that the "utopian impulse" appears not only in books but also in "goods and shop windows, traffic and advertisements, stores and the world of communications, news and packaging, architecture and media production," in short, in every corner of modern cultural life.

That utopianism is an impulse expressed with increasing intensity in the modern era is implicit in the book's triptych, which sets out from Saint-Simon, Richard Owen, and Charles Fourier, winds its way through Karl Marx and Friedrich Engels, the Marxists of the Second International, Bloch and Herbert Marcuse, and arrives at Rudolf Bahro and André Gorz. One may of course quarrel with some of these choices, especially at Bahro and Gorz, but they do serve Geoghegan's purpose, which has as much to do with his title's "and" than with either of its "isms." Geoghegan wants "to argue for a self-consciously utopian Marxism" (p. 7). He does not defend Marxism *per se*; indeed, the two contemporary writers he considers have each in important ways repudiated Marxism. But he clearly thinks Marxism would be better off if it took utopianism more seriously. Marx and Engels overreacted against utopian socialism because of "their desire to stress the political distinctiveness of their own [methodological] stance" (p. 29). The resulting distinction between utopian and scientific socialism, which by the time of the Second International had made utopianism a "mortal sin" (p. 35), "has, on balance, been an unfortunate one for the Marxist tradition" (p. 134).

Many readers will be sympathetic with this argument, especially since the "scientific" side of Marxism has proved so vulnerable to history. But the weaknesses of Marxist utopianism also have to be faced. Where in the world today is the utopian impulse in Marxism being celebrated? Geoghegan argues that utopian thinking is important because it "asks the most awkward, the most embarrassing questions" (p. 2). Yet today these seem less often to be coming from Marxism than to be pointed at it. Indeed, the whole question of utopianism now is enveloped in a paradox completely ignored here. For, although utopian impulses can be located in virtually every sort of cultural expression, their struggle to be released from these expressions seems perpetually frustrated. Jürgen Habermas has recently confessed his sense of the "exhaustion of utopian energies," his fear that "the future is occupied with the merely negative." This may of course be unduly pessimistic. Yet it is surprising to pick up a book on utopianism today in which this issue is not engaged.

The value of this work, then, lies in its cogent summary of the history of the modern utopian tradition rather than in what it aspires to be, that is, a persuasive call for a new turn in Marxist theory.

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MICHAEL JOSEPH SMITH. *Realist Thought from Weber to Kissinger*. Baton Rouge: Louisiana State University Press. 1987. Pp. xii, 256. \$30.00.

Most political scientists studying international relations accept the label "realist." The nation-state is their unit of analysis. States are assumed to act on rational calculations of interests. Interests are assumed to equate with power. Most American political leaders also pose as "realists," defending foreign policy positions in terms of "national interest."

In *Realist Thought from Weber to Kissinger*, Michael Joseph Smith describes how this came to be so. He sees Max Weber as the founding father. More insistently than Thucydides or Machiavelli or Thomas Hobbes, Weber contended that statesmen *had* to countenance evil in order to serve the state. They could not obey an "ethic of conviction" when it conflicted with an "ethic of responsibility."

After World War I, writers such as Arnold Toynbee and James T. Shotwell denied that this was any longer true. Smith characterizes these writers as "Idealist Provocateurs." Contemporary "realism" he traces to E. H. Carr's *Twenty Years Crisis*, which argued in 1939 that events between Versailles and Munich demonstrated how statesmen obeying their private consciences had ended up doing evil.

The bulk of Smith's book is about four Americans who followed in Carr's line: Reinhold Niebuhr, Hans Morgenthau, George Kennan, and Henry Kissinger. Though from different standpoints—Niebuhr as theologian, Morgenthau as international lawyer, Kennan as diplomat, and Kissinger as historian (in self-characterization)—each warred against a supposed American tradition of legalism, moralism, and isolationism. Each sought also to persuade fellow Americans that "good" ends could require ruthless use of political, economic, or even military power.

Smith's chief thesis is that "realist" definitions of "interests" actually embody values. Carr in 1939 judged Munich "realistic." Not so later. Morgenthau in the 1960s became a critic of the militarism he had encouraged in the 1950s. Kennan repudiated his views. Kissinger the national security adviser rationalized positions much at odds with those espoused by Kissinger the professor ("*Ur-Kissinger*," in the phrase of Stanley Hoffmann, Kissinger's former Harvard colleague and Smith's mentor). Smith appeals to disciples of the "realists" to recognize that ethics of conviction actually permeate ethics of responsibility.

Smith starts but does not pursue some other lines of criticism. He notes, for example, that Weber did not distinguish sharply between competition for power inside and outside states. The American "realists" ignored the internal struggles.

Smith says little about the effects of this neglect. Nor does he probe how "realist" values reflected ideology. Historians attuned to cold war "revisionism" and "post-revisionism" may be disappointed that Smith says so little about such contexts. Political scientists may fault Smith for failing to deal with other "realists" arguably more reflective or more influential, such as Martin Wight, Raymond Aron, Alexander George, or Kenneth Waltz.

Smith could not have completely satisfied either historians or political scientists without writing a much longer book. There is no exegesis of these thinkers in anything like the same compass. We should be grateful to Smith for being concise. *Realist Thought* is even-handed and always clear. It is exactly the right kind of book to introduce undergraduates to the body of international relations theory most in evidence in the decades 1940–1970.

ERNEST R. MAY
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PAUL KENNEDY. *The Rise and Fall of the Great Powers: Economic Change and Military Conflict from 1500 to 2000*. New York: Random House. 1987. Pp. xxv, 677. \$24.95.

Few books are as timely as this one. Besieged by seemingly insoluble national and international problems, Americans are eager to listen to anyone who can explain not only how the world came to be what it is but also what it might become.

Central to Paul Kennedy's explanation is his belief that productive economic forces remain the prime mover of history. According to him, nations have risen, won their wars, and achieved greatness because their military power rested on a superior economic base. Great powers become not so great when they fall behind economically. Because disparate economic and technical change is a fact of life, no great power has remained at the front for long, partly because being at the front has carried with it its own retribution. The more powerful a nation, the more it has expanded; the more territory it has had to protect, the more it has spent on defense and the more likely it has been to neglect the flourishing economic and industrial base from which it sprang. The more overextended it became, the more its power diminished.

To reach these conclusions, Kennedy has made a most detailed and exhaustive analysis of the shifts in power during the past half millennium. At the outset, he is at pains to demonstrate the earlier predominance of Asian empires such as the Ming, the Ottoman, and the Mogul. He also elaborates on the "European Miracle," that is, the

manner in which the European powers obtained mastery in the world. Thenceforth, from the sixteenth century on, he relates the rise and fall of one great power after another—the Habsburg States, the Netherlands, France, Britain, Germany, the United States, the USSR, China, and Japan—down to the present day. The last part of the book addresses the problem he entitles “Strategy and Economics Today (1943–1980) and Tomorrow.” The final twenty-two pages are given up to the question of the day: “The United States: the Problem of Number One in Relative Decline.” It is this issue that has aroused public interest in the United States.

The essential difference between this book and the works of other world historians that have preceded it (including the writings of William McNeill, L. S. Stavrianos, and my own study of the struggle for world power) is the pattern that Kennedy claims exists between a great power's relative economic strength and its ability to maintain its preeminence in the world. The ground he covers is familiar; the approach is not. Kennedy has introduced a new historical formula: $p=p$; that is, productivity equals power. Regardless of the complexity of historical causation, he believes that it is the nation with the greatest material resources that must triumph.

However oversimplified this thesis may appear, there is much in these pages for which one must be grateful. The book is important because of its scope, its theme, its sweeping conceptions, its care for detail, its intriguing patterns and parallels, its insights, and its learning. It takes tremendous industry to carry through an ambitious economic survey of world history such as this. In these days of fragmentation and specialization—to say nothing of the reluctance of some historians to use the past to illuminate the present—it also takes considerable courage.

Yet I must confess to certain misgivings. I am not happy about Kennedy's general theoretical approach. I wonder if it accurately describes the past or is relevant to the present and the future. The correlation between economic and military factors has long been recognized, but few historians, other than the Marxists, have been prepared to argue that the economic factor has been overriding. I wonder if he has not oversimplified an extremely complicated relationship. True, his theory bears up at the extremes: guns will defeat those who throw stones, but there are other examples in history that are much more problematic. Some examples seem to contradict what he is saying. Japan and the European Economic Community are strong economically but weak militarily. China is strong militarily but weak economically. The USSR has been described as a Third

World country with First World weapons. Some say it has been “overstretched” since 1917. South Korea, by far the strongest economically, lives in fear of being overwhelmed by North Korea. There are occasions, from the battles of Marathon and Salamis to the war in Vietnam, where Kennedy's analysis simply does not stand up. Could it be that he has become a prisoner of his own theory? I realize that he intends his work to be regarded as an “economic interpretation” rather than as mechanistic, “economic determinism.” On this score, I have taken careful note of his many caveats. I still think it's spinach.

It seems odd that an economic historian should scold a diplomatic historian for having stressed the economic factor. The economic dynamic is indispensable in a study such as this, but I suggest that it is only one among several. Kennedy's “doff of the cap” to other factors is simply not good enough, not in a book that purports to deal with the struggle for world power.

Ultimately, Kennedy's thesis depends on how one defines power. If power is the ability to impose one's will on others, then Kennedy is leaving much unsaid—much too much. The intangible forces of life, which have often determined the outcome of the power struggle, are largely ignored; the ambiguities of human motives and purpose tend to be forgotten. Not least important, his concentration on the power of the purse and the power of the sword has led him to ignore the power of the word. There is almost nothing here about Martin Luther, Mahatma Gandhi, Vladimir Lenin, the Ayatollah Khomeini, or others who, without a flourishing industrial base, have changed the course of world history. With the resurgence of theism, especially with the growing force of religious fundamentalism in the Middle East, it may well be intangible forces that will decide tomorrow's conflict.

Kennedy gives the impression of being more concerned with statistics than with people. It is almost as if that which cannot be counted does not exist. The danger of such an approach is that one loses sight of the fact that counting is not causation. Worse still, one loses sight of human beings. From all the recognition they receive here, Wellington might never have been at Waterloo nor Lincoln in the White House. Of course, it is much more difficult to evaluate the role of human beings than it is to extrapolate statistics.

Yet, as the author surely realizes, there is more to history than statistics. Eastern Europe fell to the Soviets in the 1940s not because of Russia's economic and military strength but because of its strategy, to say nothing of Stalin's determination. The Japanese rose in the nineteenth century partly because they were able to copy European

technology. Now the tables are turned, and we are talking about learning from them. Yet we cannot copy them as they copied us because much of Japan's present strength springs from intangible human qualities. In the last resort, it is people that make up the power of a nation. People are the greatest resource that any nation possesses.

Having worked at the historian's bench for almost half a century, I am acutely aware of the uncertainty of the human lot. There is very little in life that is predictable or inevitable. Even assuming that Kennedy is correct in his analysis of the past half millennium, it is surely unwise to extrapolate our unprecedented Western experience into the future. For the United States, it could well be the relative decline that he predicts, or it could be the beginning of an American golden age. Other people's history—think of the Chinese or of the British—has been starred with desperate occasions when disintegration seemed certain. "And the end man looked for," said Euripides, "cometh not, and a path is there where no man thought." History is oblique.

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JOHN LEWIS GADDIS. *The Long Peace: Inquiries into the History of the Cold War*. New York: Oxford University Press. 1987. Pp. ix, 332. \$24.95.

Since 1945, in some seventy wars there have been over eight million deaths. The war may have been "low intensity" or "high intensity"; it may have been someone else's war that served as a vehicle for an indirect Soviet-American confrontation or an internal revolution caught up in the great power confrontation; it may have fallen under some other euphemism for war—no matter which way, a vast number of those deaths are cold-war casualties. How then can the cold war be labeled "The Long Peace"?

The stratagem John Lewis Gaddis employs is argument by definition. Peace is defined, by implication, as the absence of a full and direct Soviet-American military conflict. That accomplished, the author offers a hymn to the possibility and seeming reality of practical, geopolitical, great power peace. Read as a solid, well-documented, literate, but limited inquiry into the geopolitical tactics of high-level policy makers and crisis managers during the Truman and Eisenhower years, Gaddis's work has real value. How the two seeming mortal enemies avoided direct warfare for the fifteen years after World War II is a tale worth telling. But, read as an explanation of the nature and dynamics of the cold war, the essays fall short.

Much of the book is a critique of the effect of

ideology on relations between the two nations. Beginning with a broad survey of Russian-American relations to World War II, Gaddis argues that "antagonism between our two countries has most often arisen . . . from attempts by one side to somehow change the internal system of the other" (p. 18). This is part of a well-crafted argument that geopolitical cooperation has worked, that ideology is not a necessary part of that relationship, and that political economy is (or should be) subordinate to geopolitical considerations. Thus, in the second essay, Soviet-American relations during World War II are used to demonstrate that, if ideology is ignored, geopolitical realities will permit cooperation—a position embraced in recent years by Soviet historians. Gaddis mentions sources of distrust and dispute that were held at arms length during the war, but the thrust of his presentation is that much of American wartime policy toward the Soviet Union was shaped by practicality, not ideology.

The main body of essays presents a Eurocentric view of a number of intriguing geopolitical themes. An essay on defensive perimeter strategy in East Asia prior to the Korean War finds the United States the prisoner of other forces. American involvement in that conflict came out of politics and credibility, not a conscious strategy. Gaddis's discussion of the "third force" concept in postwar Europe presents a fascinating picture of George Kennan and Charles Bohlen switching positions on spheres of influence and power politics. The only piece, other than the title essay, that ventures beyond 1961 documents how reconnaissance satellites have been left alone by conscious though unofficial mutual consent, because the information satellites provide about military intentions promotes rather than diminishes the two nations' sense of security. This argument may make government officials nervous, for the same logic could apply to leaks of classified information.

But that brief excursion is all there is on the last twenty-five years of the cold war, the period when internal revolution and nationalism reshaped and redirected what had been a bipolar confrontation. The abrupt halt in 1961 makes the vast struggle in Indochina virtually invisible. Despite the availability of a vast storehouse of archival sources, there are only three random references to that conflict. Nowhere does Gaddis analyze or attempt to fit in or even explain away our Vietnam War. Granted, Soviet and American troops did not face each other across the trenches (or the tunnels), but to ignore that war while inquiring into the history of the cold war defies understanding—and the realism that Gaddis praises. To ignore Vietnam is to trivialize the cold war.

There are other, less important, but nonetheless

jarring notes. Gaddis writes with undisguised glee when he finds evidence that denies economics as a motive in American foreign policy. That is a minor if aggravating habit, but it reveals a more serious shortcoming. Before his self-appellation of postrevisionism can be believed, Gaddis must go beyond arguing against the revisionism of the 1960s, with all its shortcomings, and confront the intellectual arguments of postrevisionists whose interpretations emphasize political economy in its broadest sense. Economics in foreign policy cannot simply be equated to greed and profit. "Capitalism" as used by such scholars is not (or should not be) an epithet but rather a descriptive phrase that combines politics and economics. To ignore geopolitics is foolish. To make geopolitics into a form of determinism is equally mistaken. Geopolitics, for Gaddis, is an ideology. But that fails to confront the liberal Americanism that underlay U.S. policy.

This book explains why American policy makers thought as they did about the immediate, short-term challenges posed by the Soviet Union. War, death, and destruction always take precedence in people's minds over long-term issues of systems and nationalism. But the broad, fundamental questions of an American philosophy, of geopolitics as the tactics of a strategy created by a nation's political economy, are evaded and avoided. Despite Gaddis's references to the work of political scientists, he fails to confront the challenges to Realpolitik raised by scholars such as John Burton, J. David Singer, John Vasquez, and others. Where is Gaddis's theory of expansion? He accurately identifies the expansion in 1949 of the American strategic frontier in the Pacific from the West Coast to the offshore islands of Asia, but why? Did only geopolitical strategy underlie General Douglas MacArthur's arguments or American reactions to Mao Tse-tung's victory in China? The "China Myth" and its variations and combinations never make an appearance.

I could praise dozens of small points and quarrel with dozens more. But, ultimately, the assumptions behind the work leave me disappointed. The multiarchival research into geopolitical relationships is excellent. But that narrow focus is not sufficient as a guide to the history of the cold war. We are all thankful that a direct, global Soviet-American war has not broken out since 1945. Perhaps, as Gaddis suggests, the tactics of American leaders were wise, or perhaps they were just lucky. What remains is to determine why.

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HUGH THOMAS. *Armed Truce: The Beginnings of the Cold War, 1945-46*. New York: Atheneum. 1987. Pp. xviii, 667. \$27.50.

To the substantial body of historical literature on the origins of the cold war we can now add this elaborate account by British historian Hugh Thomas. Well known for his works on the Spanish Civil War and Cuba, Thomas comes to this challenge well respected for his earlier achievements. He promises us that this book is but the first of several that will examine the cold war era. Thomas operates from the assumption that understanding the personalities of national leaders of the major nations is the key to comprehending the complexities of this crucial time. He offers dozens of character portraits, most of which are very ably sketched. I found the coverage of American leaders the most interesting, but Thomas is to be commended for his analysis of many Allied leaders, something that other writers on this period have often skimmed. In relating his biographical treatment to policy making, Thomas is strongest when he covers the Soviets. This book is clearly one of the best available in laying out the forces that influenced Soviet decisions.

Armed Truce offers readers a good balance. Thomas does not overdo the story of one nation at the expense of another. He provides extensive treatment of the territorial questions at war's end, bringing in countries of the Middle East as well as China, Japan, and Korea. Even in this lengthy book, though, Thomas has space only for quite limited analysis of non-European regions. He offers a traditional interpretation of why the cold war started. "The prime cause of the conflict," he concludes, "opening up between the Russians and the Americans (and their allies) was the ideology of the Soviet leaders" (p. 548), which kept them from making permanent arrangements with the capitalist nations. Coupled with ideology, he writes, was the Russian desire for security against a third German attack. Thomas takes pains to refute the notion that the Americans tried to exercise any form of atomic diplomacy. Otherwise, he covers none of the other revisionist arguments about the nature of the cold war, despite ample evidence that American leaders gave considerable thought to how they might use victory to expand overseas markets and investments, which wartime influence had made possible.

The author has explored memoirs and secondary literature extensively. He also makes excellent use of materials in the files of the British Foreign Office and in the State Department records in the National Archives. He includes some citations from the Harry S. Truman Library but, oddly, nothing from the Roosevelt papers. Thomas can

write well. At times his account is lively and vivid. Unfortunately, there are too many occasions when sentences are convoluted. This book deserves sharper editing. For all its many merits, *Armed Truce* sheds little new light on its subject, which is a shame as Thomas put in a great deal of effort to that end. But anyone who has read the work of historians such as Herbert Feis or John Lewis Gaddis will come away after reading this volume wondering why it was written.

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ROBERT CHADWELL WILLIAMS. *Klaus Fuchs, Atom Spy*. Cambridge, Mass.: Harvard University Press. 1987. Pp. vi, 267. \$25.00.

Robert Chadwell Williams has written a scholarly account of the career of Klaus Fuchs, the refugee German Communist who, as part of the British contingent working on the Manhattan Project, gave nuclear secrets to the Soviets. Having found a wartime report by Fuchs among items sent to the Soviet Union, the Federal Bureau of Investigation tipped off British counterintelligence. Carefully coaxed (to avoid revealing to the Soviets that their code had been cracked), Fuchs confessed "what was once called, and may still be," the author asserts, "the crime of the century."

Circumstances of his youth inclined Fuchs toward communism. A Lutheran minister turned Quaker, his father became a prominent German Socialist. As the Nazis rose to power, his children, Klaus included, gravitated toward Germany's Communist party. After Adolf Hitler took over, Klaus, under party orders, fled to Britain, where he continued to study nuclear physics. For a time he was interned, but his skills in theoretical physics won him reprieve from internment, a role in Britain's A-bomb project, and then a job at Los Alamos. By late 1941, he was delivering reports to the Soviets.

British security blithely dismissed an early report of Fuchs's loyalties (because it came from the Gestapo). Williams traces Fuchs's links to a spy ring run by refugee German Communists and the ties between that group and elements of the anti-Nazi "Lucy" network operating out of Switzerland. These linkages are complex. Williams suggests that Fuchs' perfunctory security check may have owed something to Soviet penetration of M16. The connection between the Fuchs case and some of these instances of the breakdown of British security is not always clear, but Williams argues that it shows the interpenetration of British and Soviet wartime intelligence and thus an additional hidden motive for subsequent British reticence about the case.

In 1944, soon after arriving in the United States, Fuchs made contact with his courier Harry Gold. He passed on data about the gaseous diffusion process and implosion. He had some familiarity with the emerging theoretical foundations of the hydrogen bomb, but the fear, widespread when the Fuchs case erupted, that the Soviets had vaulted ahead in the race for the H-bomb was baseless.

Fuchs returned to England in 1946 to take up an important role in the British A-bomb project. In October 1949, when his father decided to move to East Germany, Fuchs told the project security officer that he feared this development made him a security risk. He was already under suspicion. He confessed his espionage activities in January 1950. Tried in March and sentenced under the Official Secrets Act to fourteen years in prison, he served until 1959 and then moved to East Germany.

The book is based on wide research in British, American, and Canadian governmental files. Unfortunately, Fuchs did not leave a heavy biographical trail, so the result is often the biography of a case more than of a person. Williams astutely analyzes the episode's political impact. It torpedoed U.S.-British nuclear cooperation, accelerated the American race to build the H-bomb, and strengthened the hands of those wishing to tighten security in American science. On the British side, the case was handled gingerly. Fuchs's confession remained confidential: its disclosure would have revealed that Soviet agents had operated on British soil during the war and might have exposed such Soviet moles as Kim Philby and Anthony Blunt.

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ANCIENT

M. I. FINLEY. *Ancient History: Evidence and Models*. (Elisabeth Sifton Books.) New York: Viking. 1985. Pp. 131. \$17.95.

This collection of six essays published between 1977 and 1985 focuses on the issues stated in the title: the problems inherent in the evidence available to ancient historians and the use of the nonmathematical model ("a simplified structuring of reality which presents supposedly significant relationships in a generalized form" [p. 60]) to overcome these problems. Much of the book is framed in the context of earlier historiography, which sometimes reflects the occasion of the orig-

inal publication, and M. I. Finley's debt to Max Weber is prominent.

The topic is timely. In the past, models have been most often championed by adherents of the "new archaeology," but a growing interest among ancient historians is signaled by the NEH Institute, jointly sponsored by the Association of Ancient Historians and the University of Washington in the summer of 1987, and by the choice of "The New History and the Old" as the topic for the Friends of Ancient History panel in January 1989.

The strongest aspect of the book lies in those chapters that point out the fragmentary and problematic nature of ancient sources. Thus, Finley argues in chapter 2 against the privileged position often accorded to ancient literary sources despite their quality, and in chapter 3 he stresses the slight foundations upon which population estimates and other ancient "statistics" are based and discusses the ways in which existing documentary records were biased by the aims of the collectors of the information. Yet probably few historians today would lay claim to the positions that Finley attacks in chapter 4: that Thucydides (or any other historian) produced objective, value-free history and that oral tradition as used by ancient historians can be swallowed whole.

More questions arise when we proceed from the critical parts of the book to its positive message of the utility of models. In chapter 5, Finley sketches the outline of a model for studying the role of war in ancient societies. The end product is a twofold classification of ancient states: marine empires controlled by the *demos* and land empires controlled by an oligarchy. But does such a schematized skeleton model, derived in essence from Finley's prior conception of imperial Athens and Rome, increase in any substantive way our understanding of these or other ancient states? Although Finley is caustic in his criticism of histories of individual cities and areas, it is only this sort of work that can provide meat for these bones.

Finley claims that models are tentative, always undergoing revision and improvement. But the strength of a model once created is emphasized in chapter 6 as Finley struggles to fit Athens into the model of Weberian charismatic *Herrschaft*. Ultimately he finds the model unsatisfactory, but a non-Weberian is tempted to question whether the prolonged effort to escape from the model was productively spent.

The seductive persuasiveness and staying power of models are major reasons for the importance of this book. There is a persistent danger that models, now often incorporated into archaeological reports (for example, C. Renfrew and M. Wagstaff, *An Island Polity* [1982]), will be mistaken for fact by ancient historians if they are not clearly

aware of the methodology. Thus, this brief book can be useful not only for those who use models or contemplate their use but also for those who still view history as the rich stew of facts that Finley labels "antiquarianism."

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JOSINE BLOK and PETER MASON, editors. *Sexual Asymmetry: Studies in Ancient Society*. Amsterdam: J. C. Gieben. 1987. Pp. ix, 298. f 70.

This collection of nine essays includes six that were previously published in Dutch in 1984. Together the essays span more than a thousand years of social history, from Homeric society to late Roman antiquity. In this volume, as in other recently published collections devoted to gender relationships in classical antiquity, articles on Greece predominate. The contributions of the two editors, Josine Blok and Peter Mason, occupy more than one-third of the entire book. Most of the essays begin with lengthy reviews of secondary works and end with exhaustive bibliographies, including a substantial number of books available only in Dutch.

Blok's "Sexual Asymmetry: A Historiographical Essay" serves as an introduction to the entire volume and offers as well a unique discussion of the place (or lack thereof) of women's history in the work of nineteenth-century classical scholars. In her criticism of contemporary methodological approaches to gender studies, including those adopted by some of the authors of the other essays, Blok singles out structuralism as a system that regards male and female as polarized categories and consequently denies the asymmetry and hierarchization of the sexes. Although Blok reiterates her description of the collection as "gender studies" rather than as "women's history," most of the essays put women at the center of the inquiry, use feminist perspectives, and would fit comfortably into the category of feminist studies.

In "Wife and Helpmate: Women of Ancient Athens in Anthropological Perspective," H. S. Versnel presents yet another historiographical survey of ideas on the status of Athenian women that have been advanced by scholars in the twentieth century. He quite correctly criticizes Blok for giving short shrift to the influence of ancient authors on the views classical scholars hold on gender in antiquity and for overemphasizing the effect of nineteenth-century ideology.

Ruth Oldenziel's "The Historiography of Infanticide in Antiquity. A Literature Stillborn" is a review essay of scholarship on infanticide from the late nineteenth century to 1983. This article could

have benefited from attention to demographic data from Rome and Egypt and from comparisons with anthropological reports on methods of population control in underdeveloped areas.

F. G. Naerebout, in "Male-Female Relationships in the Homeric Epics," takes a step backward in current scholarship on social relationships in the Bronze Age by refusing to use linear B texts "because they have hardly anything to do with the epics" (p. 112) and then offering an analysis of the epics resembling that published some twenty years ago by W. K. Lacey in *The Family in Classical Greece* (1968). The doubt Naerebout expresses about the prevalence of endogamy in classical Greece is surely disproven by the institution of the *epiklerate*, which compelled close kin to marry, as well as by studies of marriage patterns in Athens by Wesley Thompson and Cheryl Cox.

In "Third Person/Second Sex. Patterns of Sexual Asymmetry in the *Theogony* of Hesiodos" and in "Post Scriptum. Hesiodos-Nietzsche-Derrida," Mason deconstructs the myth of Pandora at length. An ahistorical approach distinguishes this article from the rest of those in the volume.

In a ground-breaking article, "The Old Women of Ancient Greece," Jan N. Bremmer actually discusses both Greek and Roman misogynistic literary texts. His definition of "old" seems to be "postmenopausal." One wonders, however, how ancient critics could have known whether a woman had reached menopause. A discussion of the meanings of words such as *graus* (old woman) and of the average age of menopause as given in the medical literature would have been relevant. Contemporary anthropological studies of Greece that report both the disdain and the freedom accorded to postmenopausal women would also have shed some light on the past. Bremmer asserts that women in some Greek cities were prohibited from drinking wine but does not supply any documentation for this surprising statement. In fact, medical texts often prescribed wine for women's problems, and the depiction of little girls on *choes* (wine jugs) indicates that even in Athens women drank, though, to be sure, there were plenty of jokes about female dipsomania. In ascribing asexuality to old women, Bremmer ignores the lust for virile young men attributed to old women in diverse works of fiction such as Aristophanes' *Ecclesiazusae* and Apuleius's *Metamorphoses*. In concentrating on the negative attitudes of Greek and Roman authors toward old women, Bremmer ignores a positive factor. Old women were protected by laws and customs that prescribed that their children look after them. Bremmer's article is especially valuable for its provocative obiter dicta: for example, "The arrival of literacy enabled the male Greeks to circumscribe

women's freedom of movement in a much more precise and compelling way than would have been possible in a completely oral age" (p. 192).

Emily A. Hemelrijk in "Women's Demonstrations in Republican Rome" goes over familiar ground, pointing out that Roman society was characterized by class solidarity rather than sexual asymmetry. She draws attention to the solidarity displayed by upper-class women who, relying on their rank and status, organized five effective public demonstrations. Hemelrijk hypothesizes that in the empire the *conventus mulierum* (assembly of women) became a formal channel through which women could express their opinions.

Jan Willem Drijvers in "Virginity and Asceticism in Late Roman Western Elites" builds on the publications of Averil Cameron and Rosemary Ruether to suggest some ways in which women's asceticism was advantageous for Christian men.

Last but not least, Marianne Maaskant-Kleibrink in "Nymphomania" marshals evidence from the visual arts and Western literature dating from classical antiquity to the present to trace the transformation of the figure of the nymph. Originally a gift of the gods, the nymph evolved into the pathological protagonist of contemporary pornography. Maaskant-Kleibrink's article serves as an exemplary demonstration of the use of the present to illuminate the past.

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MARTIN OSTWALD. *From Popular Sovereignty to the Sovereignty of Law: Law, Society, and Politics in Fifth-Century Athens*. Berkeley and Los Angeles: University of California Press. 1986. Pp. xxii, 663. \$75.00.

This eagerly awaited book traces the "growth of popular power in Athens to the point at which it became popular sovereignty" (p. xix) by examining "the delicate interrelations between intellectual trends, social currents, and political events" (p. xi). The various chapters can be used independently, but every piece contributes to the larger argument, and even discussions that might at first glance seem to cover familiar ground move toward specific and often exciting ends. At the very least, even with the dustiest topics of fifth-century political history, the scholarship is so thorough and the footnotes so clear and complete that Martin Ostwald's discussion will be an indispensable reference tool; more often than not, however, the intellectual energy brought to each issue is so great that new light is shed on at least some aspect and stale problems are given new life.

Part 1, which sets the stage for the democracy associated with Pericles, consists of three chapters. The first outlines the critical political or constitutional reforms of the first half of the fifth century. Here is a discussion of Cleisthenes' reforms that finally creates a convincing picture of their balanced aims and the reasons for their effectiveness. Ephialtes' reforms, commonly held to be the next stage, are here anticipated by an intermediate stage that Ostwald bases on a fascinating interpretation of an inscription republished in the late fifth century (*Inscriptiones Graecae* I, 3, 105). The second chapter, devoted to social thought and values related to law and government, seems the weakest in the book. Ostwald relies on a distinction between descriptive and prescriptive uses of various terms important in law and ethics, but these uses seem to have overlapped considerably and require a more complex analysis of areas of usage. From an institutional viewpoint it is useful to distinguish more clearly between the Areopagus and the popular courts: all dealt with legal settlement, but they had radically different histories giving them very different relationships to the tensions between descriptive and prescriptive norms. Finally, the conclusions about the meanings of some terms seem more specific than the scattered data will permit. The final chapter in this first part uses four texts as milestones in the fifth century through which to trace a certain secular encroachment on religious authority. Although the development probably did take place during this period, the cases chosen seem too diverse in tone and context to form a satisfactory basis for a comparative discussion.

The three chapters of part 2 begin with a description of the state of government in the mid-fifth century. Then the intellectual trends most associated with the Sophists are given a detailed treatment. Throughout, broad intellectual topics are tied closely to specific texts and political figures. Passages are treated carefully—the discussion of Plato's *Gorgias* 483 b–e is especially sensitive—and here, as throughout the book, every important citation is given both in Greek and in translation. This section successfully accomplishes the difficult historical task of creating a convincing picture of real connections between intellectual history and political and military events. The ideas of aristocratic Athenians and the events surrounding Melos and Sicily during the Peloponnesian War are brought into plausible proximity with a fresh defense for the historical accuracy of Thucydides' account of the Melian dialogue.

Part 3 examines the events surrounding oligarchic revolutions at the end of the fifth century and the restoration of a constitutional democracy. The

general picture of the creation of a more self-conscious legal and constitutional order in response to a lawless oligarchy will not be controversial. But here, too, Ostwald's reexamination of the details creates new suggestions for the nature of the Arginusae trial and for various aspects of the events leading to and following the revolutions.

If some of Ostwald's reconstructions seem to go beyond the evidence, especially in their specificity as to date, the ability to question the discussion is always provided by the author himself. Ostwald is completely honest about the limits of the data and sources and never tries to present interpretation as fact. His evaluations of the primary sources and the secondary scholarship are always balanced and judicious. Even when his interpretations require a chain of assumptions, each link is always probable and plausible: conclusions are never reached by a string of unlikely possibilities. This full examination of the development of politics and government in the fifth century is both sufficiently novel to make it exciting and so thorough and sound that it will be not only useful but probably necessary for decades to come.

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J. S. MORRISON and J. F. COATES. *The Athenian Trireme: The History and Reconstruction of an Ancient Greek Warship*. Paperback edition. New York: Cambridge University Press. 1987. Pp. xxiii, 266.

How the Greek trireme with three superimposed banks of rowers could fight, move, or even float has been debated for centuries. When, therefore, a replica of such a ship was recently launched and commissioned as a unit of the Greek fleet, the event attracted much attention in the world news media. Here the guiding spirits of the project, a classicist and a naval architect, lay out the historical research and technical planning on which the reconstruction was based, with J. S. Morrison providing the research and J. F. Coates the planning.

Initial publicity secured funding for a mock-up and trial piece; the formation of a "trireme trust" and financing from the Greek navy made possible the building of the full-scale replica in Greece. Since no trireme or description of one exists, any attempt to reconstruct must begin with a thorough study of the ancient evidence, written and monumental; the results of this theoretical reconstruction must then be tested by the finished replica. Omitting the first step, previous attempts have failed because of a tendency to explain triremes in terms of contemporary ships (for example, the *alla sensile* and a *scaloccio* galleys). The belief that *tetrereis*, *pentereis*, and others had four, five, or

more banks of oars differing in length (a practical impossibility) also cast doubt on the feasibility of triremes. Yet representations discovered in the nineteenth century proved that three-level ships existed. They were the descendants of one-level Bronze Age vessels that evolved into two-level galleys with the same oar power but reduced length, which gave them the speed and agility for new ramming tactics as opposed to boarding and fighting on deck. The next step was the invention of the trireme. Morrison's reasonable sketch of this development is likely to be right.

This cannot be said of his treatment of eight fifth-century seafights that, as the first continuous account since Admiral W. L. Rodgers's *Greek and Roman Naval Warfare* (1937), would have been most welcome. Morrison's idiosyncratic preference, not justified by the sources, for ramming over boarding tactics leads him to exaggerate the use of ramming (for example, at Artemisium and Salamis), while explaining away boarding (at Lade and Sybota). Ramming was effective only in open waters with room to maneuver. Commanders soon learned to avoid such battles; the problem of maneuverability and possible damage to the ramming ship itself caused the supersession of the trireme, contrary to Morrison's speculations. These chapters constitute a regress from the excellent analysis by the experienced naval commander Rodgers.

Morrison correctly stresses that the crews of triremes—the ships had a limited cargo capacity—usually had to stay close to land in order to get food and water, although for special operations they could carry enough to be self-sufficient. The conclusion that the speed of a trireme varied between five and eight knots is eminently sound. Less satisfactory is the study of crews. Morrison makes the facile assumption that the technical term *hyperesia*, derived from the word for rowing and with pejorative connotations, refers not to the rowers, whose lowly social status it fits, but to the ship's officers and the marines who belonged to an upper class. The effort to extract a maximum of information from often unyielding sources about the configuration of the ship's features, including the arrangement of rowers and oarports, is generally successful. Speculation is here kept to a minimum, as it is in the investigation of the timber and other materials used in ancient shipbuilding. Coates's exposition of the principles of design and reconstruction, accompanied by excellent drawings and diagrams, is lucid and convincing. Two technical appendixes, also by Coates, calculate the speed of fast triremes and analyze the angular perspective of the Lenormant Relief, a chief piece of evidence. The book is very well illustrated and

equipped with excellent glossaries, indexes, and maps and a good bibliography.

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HERBERT SCHUTZ. *The Romans in Central Europe*. New Haven: Yale University Press. 1985. Pp. viii, 200. \$25.00.

Recent archaeological work along the Rhine and Upper Danube remains largely inaccessible to those who do not read German, and a good synthesis in English is much to be desired. This is not it. Although claiming to be "an authoritative overview," it relies almost exclusively on second-hand sources and works of popularization. The bibliography contains not one single excavation report and only two articles from scholarly journals; instead, we get *Köhner Römer Illustrierte* and Paul MacKendrick's *Romans on the Rhine*. Names are distorted: Christopher and Sonia Hawkes become Hawks, Mortimer Wheeler is improbably disguised as Robert E. R. Wheeler, and Edith Wightman more than once turns up as M. E. Wightman. Mistakes like this do not inspire confidence; nor does the appearance on page 5 of Marcus V. Agrippa.

Chapter 1 is entitled "The Military Setting." The scholarship is haphazard. The amber route through Carnuntum was no secret even before Werner Jobst's "repeated references" to it in 1983, and Tacitus, not Graham Webster, first suggested that Germanicus was popular with the troops. But Jobst and Webster get the credit because Herbert Schutz happens to have read their books and no deeper. On the crucial question of Rome's overall strategy, it is not self-evident that Roman generals were "intent on stabilizing the populations of central Europe" (p. 7), and E. M. Luttwak, *The Grand Strategy of the Roman Empire* (1976) cannot simply be ignored. It is over half a century since Ronald Syme pointed out the fallacy of seeing the difficult Black Forest terrain as a funnel for invaders, just because it shows as a reentrant angle on the map, and I would rather not be cited on page 8 (n. 48) as if I agreed with the view that the Rhine was "a purely defensive line" until Drusus took command, and that "conditions forced (my italics) an expansionist policy on the military commanders," when I wrote a whole book, cited over twenty times by Schutz in the notes, to argue the opposite case.

So we continue: it is not "fairly certain" that Arminius's service with the Romans was on the Rhine. Why not in Illyricum? The famous acephalous inscription (*ILS* 8965) recording campaigns

across the Danube need not refer to M. Vinicius (p. 11): Schutz neither gives the reference nor notices the discussion. On the next page, he misses the implication of Velleius's reference (2.112.2) to a *legio semiplena*: a legion going on campaign might leave part of its strength behind at its base. It is strange (p. 14) to find Varus's treatment by our sources described as "too kindly" and unforgivable to be unaware of the precedents for suicide by a defeated commander. *Victrix causa deis placuit, sed victa Catoni*. The expression *tribuni laticlavii* (sic) suggests ignorance of Latin, rather than poor proofreading. Finally, the Germans in the first century B.C. did not have stirrups; indeed, it is hotly disputed whether even the Hunnic invaders in the fourth century had them.

Chapter 2, devoted to religion, is no better. Nero's death is dated to A.D. 38 (p. 50), the popularizing *Kölner Römer Illustrierte* is cited as authority for the Romans' "open-minded attitude towards the religions of others" (p. 54), as if the idea were controversial, and the unpublished Ph.D. thesis of one of Schutz's colleagues is given credit for the seemingly novel suggestion that the Druids "retarded Gallo-Roman syncretism" (p. 56). What *will* these daring young scholars think of next? Well, on page 66, Nero seems to be personally credited with establishing peace and prosperity on the Rhine. Perhaps we misjudged him. A chapter on religion in the Rhineland cannot omit the cult of the Matronae. It would be better for Schutz's credibility if it had. On page 64 are two illustrations. Figure 54 displays the well-known dedication *MATRONIS AVFANIABVS* from the Rheinisches Landesmuseum at Bonn (inv. no. D 227). The caption correctly identifies "the Aufanian Matrons," but gets the date wrong (it is right in the text, p. 53). Next to it (fig. 53), a less well-known dedication, also from Bonn, reads in the first line *DEABVS.AVFANI*. The letters and the word division are utterly legible. The caption refers to "the Busavean Matrons." At this point, I began to doubt my sanity.

In Chapter 3, "Arts and Crafts," the discussion of terra sigillata development on page 118 is full of misinformation, partly though not entirely due to loose English. It omits the Lyons workshops altogether, implies that Trier was *a* if not *the* main center of production by A.D. 100, and suggests that "in the frontier provinces," but not in southern Gaul, manufacture was "in the hands of family units." Yet graffiti show Celtic to have been the language of the workshops at La Graufesenque, and work was probably seasonal, fitting the local rhythm of peasant life. This suggests an examination question: "How did the organization of production at La Graufesenque and at Rheinzabern differ? (Candidates are advised to read D. P. S.

Peacock, *Pottery in the Roman World*, 1982, chapter 7, before attempting the question.)" The potter's mark is called, in English, a "stamp," not a "seal," and *sigillata* refers not to this but to the figures in relief on the decorated wares. A. H. M. Jones (n. 78) is no longer the latest authority on the underlying economic factors. A discussion of the Hildesheim treasure (p. 132) introduces the statement that finds of Roman silverware are "not unusual" west and south of the *limes* and supports it by references to Plate 6c and note 118; Plate 6c illustrates some silver from the Kaiseraugst treasure, which is not otherwise mentioned, and note 118 refers, not to Kaiseraugst, as one might expect, but to an article in (once again) *Kölner Römer Illustrierte*, on Manching! "An authoritative overview"?

Misprints are few, the color plates generally good, the black-and-white uneven, the index inadequate. Note 1 proclaims the intention of referring to Caesar by the pages of Moses Hadas's translation of Caesar (New York, 1957), not in the conventional way by book and chapter. I should not let my students get away with this in an undergraduate course and am appalled to find it in a book published by Yale University Press.

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MEDIEVAL

JUDITH HERRIN. *The Formation of Christendom*. Princeton: Princeton University Press. 1987. Pp. x, 530. \$34.95.

The formation of Christendom is a basic challenge to every teacher and researcher whose interests even tangentially relate to the era from late antiquity through the eighth century, and all those who must deal with the subject should read Judith Herrin's book before returning to classrooms and narrower specialties. Herrin attempts to strike a balance between the specialist's need for detail and the general reader's requirement of a structured survey. On the whole she succeeds, although few novices will relish the extensive attention given theological questions, especially monotheism and iconoclasm, even though they are clearly important to the subject. Overall the book is closer to a very good textbook than to a stunningly original synthesis. For example, that Islam and Byzantium were locked in stalemate after the eighth century and thereby allowed the West to explore new and peculiarly Western evolutionary paths is hardly revolutionary. Nor is the idea that

the Mediterranean basin gave unity to the ancient world and that the loss of that centrality marked the birth of something no longer Greco-Roman. Indeed, Herrin begins by acknowledging Henri Pirenne and others who have written more recently. The outstanding contribution of Herrin's book lies at the next level of synthesis: the stripping away of accumulated clichés and the replacing of them within the traditional grand synthesis by carefully reasoned explanations derived from the best recent scholarship. This is particularly true for Byzantium, on which the author concentrated in her earlier work. Once Herrin moves past the era of Justinian I, her use of primary sources is much more pronounced, and, as a result, the narrative is more compelling and enjoyable, culminating with what is surely the best succinct discussion of the age of iconoclasm and the Carolingians available in English. With characteristic care Herrin elucidates the religious, administrative, and military aspects of Byzantium, the Carolingian involvement and understanding of religious issues as set out at Frankfurt in 794 and in the *Libri Carolini* and other documents, and, most important, the essential dilemma of the papacy caught in the middle by its own theological traditions and its recent political decisions.

Despite all the justly merited praise, there are occasional errors of fact, most typically drawn from the errors of secondary materials relating to the West: for example, although Leovigild may be said to have developed the first "national coinage," he was not "the first western medieval ruler to put his own name on the coinage, as *dominus noster*" (p. 226). That distinction must be accorded to the hapless Ostrogoth Theodahat whose 40 nummi bronze revealed him wearing a spangenhelmet on the obverse, inscribed *D N Theoda-hatus Rex*, with reverse of victory on prow, *victoria principum*. This and similar cases reflect a weaker control of Western materials but should not be construed as negating the thesis or any major aspect of it.

More significant is the author's unsatisfactory discussion of the role of the Roman army as a major, probably the major, structure giving cohesion to the late empire. The army's recruitment policies, rotation and transference of units and allies, the extremely important and distinctive religious life of the Roman troops, the fiscal and bureaucratic structures related to the military system, the promotion system, the frontier, and so forth, all combined to give coherence and definition to late ancient society whether Eastern or Western. In its triumph Christendom ultimately replaced, redefined, or realigned many of the features of life once perceived as relating to the Roman army. Without accepting Gibbon's classic analysis, the subject demands a discussion not

found here. Nor will the reader learn much about the archaeological dimensions of the transitions in society, again a particularly weak analysis by Herrin of the West. The traditional artistic and cultural materials are, however, nicely brought into the discussion.

In summary, *The Foundation of Christendom* offers a well-written and careful synthesis of the period from Constantine I to the middle of the ninth century, during which the continuity of Mediterranean civilization was broken into three distinct alternative evolutionary paths. The Byzantine remained truest to the ethos of antiquity and throughout the era was the basic center of cultural gravity. The Islamic world (the least discussed area) too long failed to realize its dream of replacing the Roman world centered on Constantinople and by overwhelming so many of the Greco-Roman alternatives lacked the creative friction to push beyond its early syntheses. Only in the West did society create a restless equipoise through which Christendom could build upon the dynamic amalgamation of late antiquity and barbarian societies. Although certain orientations of the discussion and occasional facts may arouse specialists, everyone should appreciate the accomplishment.

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JAMES L. KINNEAVY. *Greek Rhetorical Origins of Christian Faith: An Inquiry*. New York: Oxford University Press. 1987. Pp. xi, 186. \$29.95.

The potential reader of James L. Kinneavy's study should be forewarned that the author is claiming that the concept of Christian faith, rather than its content, derives from Greek rhetoric. Noting that in Plato and Aristotle, faith (*pistis*) is associated with opinion, sense perception, and probability, in contrast with knowledge (*episteme*), he seeks to demonstrate that in the tradition of Greek rhetoric, faith shares the characteristics of persuasion. The structure of the book is threefold: the semantic argument, which traces the specifics of persuasion in this Greek intellectual tradition; the historical argument, which wants to demonstrate the influence of this mode of thinking on the early Christian writers; and the analytical argument, which attempts to prove by detailed analysis of the 491 occurrences in the New Testament of the noun *pistis*, and the cognate verb *pisteuein*, that the concept was indeed shaped by the influence of Greek rhetoric. Since the Greek theory of rhetoric was flexible and adaptable, it was able to serve as a framework on which differing views of faith could be erected, with varying emphases in Paul, John, James, Peter, Mark, Matthew, and so on (p. 21).

The peculiar amalgam of Greek and Jewish notions in some of the pre-Christian Jewish writings set the precedent for the accommodation of Hebrew to Greek notions among the early Christian writers.

In the Greek tradition, *pistis*/persuasion functioned as both process and result: how people were persuaded and of what they became persuaded. The modes of persuasion employed by the Greek rhetoricians included appeals based on ethical (what is morally right), pathetic (what elicits human compassion), logical, and "extrinsic" factors. The last category includes appeal to authority, divine or human, beneficent or harmful. Kinneavy infers that *pistis* among the rhetoricians became what he styles as "honorific"—that is, the art of persuasion is a worthy enterprise that produces commendable results (p. 34). Building on a distinction that the Swiss theologian Karl Barth made, he sees faith in the Christian tradition as threefold: *fiducia*, trust in God; *assensus*, human assent to the value of what God is doing; and *notitia*, knowledge that builds on the other two aspects of faith. Since persuasion is central in all three of these factors, the outcome in Christianity is "the rhetorical structure of an honorific concept of faith" (p. 52). He then proceeds to show specifically how the persuasive modes of Greek rhetoric influenced both Jewish and Christian writers in the Greco-Roman period (56–100).

The third major section of the book consists of a classification of the occurrences of faith/belief in all the New Testament writings into the following categories: conversion (which includes adhesion to the new view to which one has become committed); measure of certainty (concerning the convictions); ethical, pathetic, logical, and extrinsic factors. The last of these involves the authority of Jesus, God, or the devil. By comparing these features with similar ones derived from Aristotle, the author believes he has found "overwhelming support [for] such a rhetorical interpretation of the early Christian terms, *pistis* and *pisteuein*" (p. 144).

Kinneavy has wisely and commendably noted that his analysis deals exclusively with vocabulary occurrences and ignores historical or literary context (p. 148). One might add that, in the process, he has also ignored the diversity of viewpoint among the New Testament writers—even those writing anonymously (such as Hebrews) or pseudonymously (such as the Letters of Timothy and Titus)—and the deeply altered circumstances, social and conceptual, in which documents were produced. Thus, for example, in the letters of Paul, *pistis* means trust, while in the letters written a generation later in his name, it means orthodox belief, as I Timothy 2:4–5 and II Timothy 2:18

make obvious. In Hebrews, the concept of faith has been powerfully influenced by the Platonic distinction between eternal (heavenly) archetypes and temporary (earthly) copies (Heb. 8:1–5), with the result that "faith is the conviction of things not seen" (Heb. 11:1). The Letter of James is treated as though it had been written by a disciple of Jesus, although its basic point of view, including its remarks about faith, is heavily influenced by Stoic notions. The reader of this study by Kinneavy will not have a hint of such basic distinctions that occur with regard to *pistis* among the various New Testament writers.

We can, however, be grateful to Kinneavy for his responsible sketch of the development of Greek rhetoric and for his tracing of the intellectual influence of this tradition on both Judaism and early Christianity. Although it may be true that the historical analysis "is the most difficult section of the cumulative argument" (p. 145), it is also the most substantive contribution of this study. The analytical section demonstrates, on the other hand, how artificial an analysis of texts is that ignores the changing world of thought in which they were produced. Kinneavy promises us some future studies of these themes. One hopes that he will take into account the crucial factor that this work has neglected: the changing historical context and perspectives of the early Christian writers whose work is under scrutiny.

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OTTO G. VON SIMSON. *Sacred Fortress: Byzantine Art and Statecraft in Ravenna*. Reprint. Princeton, N.J.: Princeton University Press. 1987. Pp. xv, 149; 48 plates.

Otto G. Von Simson's study of art in Ravenna in the age of Justinian first appeared forty years ago and has long been out of print. Princeton University Press has reissued the book in a paperback edition with high-quality photographs, making it available to a wider audience. Indeed, in spite of continued research by numerous scholars, there is nothing comparable on the subject in English, and *Sacred Fortress* remains a beautifully written and stimulating analysis of some of the greatest works of early Christian art.

The book is devoted to three of Ravenna's most famous churches, viewed "against the entire panorama of the age in which they were executed and in the political atmosphere of which they were a part" (p. vii). The author is not concerned with nuances of style but with art as propaganda. These works are seen as expressions of Justinian's dream of a reunified Roman empire with Ravenna as the

political and spiritual bulwark of Byzantine rule in the West.

Chapter 1 presents the *dramatis personae* involved in the creation of this art: Justinian, Archbishop Maximian, and Julianus Argentarius, who financed the projects. Chapter 2 is devoted to the Church of S. Vitale, which Von Simson sees as a victory monument commemorating the reconquest of the city from the Ostrogoths by Byzantine forces in 540. Chapter 3 analyzes the apse mosaic of S. Apollinare in Classe with its cryptic vision of the Transfiguration fused with the image of St. Apollinaris, Ravenna's first bishop. Chapter 4 discusses the ivory throne of Maximian as a product of Constantinople and a symbol of episcopal authority. The focus of chapter 5 is the Church of S. Apollinare Nuovo, built at the beginning of the sixth century by Theodoric. The original mosaic decoration, celebrating the two natures of Christ, and the subtle alterations a generation later are presented here as acts of reconciliation between the Arian and Catholic churches. Chapter 6, the weakest section of the book, summarizes Eastern and Western traits in art and ritual.

The author has not revised his text: instead, he refers the reader to Friedrich W. Deichmann's comprehensive *Ravenna*, in three volumes (1958–76). The sensitive stylistic analysis in Ernst Kitzinger's *Byzantine Art in the Making* (1977) should also be cited. The most dramatic development in recent scholarship, however, has been the realization that the early Byzantine liturgy was far more visible than the current Orthodox rite. We also know that the representation of Justinian and his retinue in S. Vitale depicts the First or "Little" Entrance when, with the clergy, the emperor entered the church carrying a gift, usually a paten, to be deposited at the altar (Thomas F. Mathews, *The Early Churches of Constantinople: Architecture and Liturgy* [1971]).

A note of caution should be added. Justinian is not mentioned in any of the dedicatory inscriptions connected with these churches, and the role of Julianus Argentarius as a surrogate of imperial policy has yet to be proved.

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RICHARD A. GERBERDING. *The Rise of the Carolingians and the Liber Historiae Francorum*. (Oxford Historical Monographs.) New York: Clarendon Press of Oxford University Press. 1987. Pp. 209. \$47.00.

Owing to the intractable nature of the sources, writing Merovingian history is invariably a "what if" game. Richard A. Gerberding plays it well, asking what if the *Liber Historiae Francorum* (LHF),

the shortest and least appreciated of the three Merovingian historical texts, were to be accepted as an accurate, trustworthy account. The result is in part a source-critical commentary on the LHF and in part a series of roughly chronological essays on perennial debates in Merovingian political history.

Gerberding describes the author of the LHF as a Neustrian aristocrat writing in the Somme valley, probably at Saint Medard of Soissons. Although Gerberding assumes him to have been a monk, his interests were entirely secular: the Neustrian Franks and their kings. His motivation was to demonstrate that the eastern Carolingian families, who at the time of his writing (727) were in control of Neustria, were legitimate rulers of the Franks as long as they governed under their Merovingian lords. This perspective and motivation set the tone of the LHF and give Gerberding his unifying theme, the rise of the Carolingian family as seen from the Neustrian perspective.

If the author of the LHF is more accurate and forthcoming than others, then immediately following the death of Sigibert III in 657 Grimoald I installed his son Childebert as king in Austrasia. Only after the latter's death at least five years later was Grimoald captured and executed by the Neustrians, who temporarily reunited the two kingdoms under Clovis II and Clothar II. The Austrasian aristocracy, probably under the leadership of Pippin II, recalled Sigibert's son Dagobert from exile in Ireland in order to recover Austrasian autonomy.

Again, if one follows the LHF, then the rise of the Carolingians in Neustria was the result less of military victories, such as the battle of Tertry in 687, than of a clever matrimonial strategy. In particular Pippin secured Neustrian support by marrying his son Drogo to the daughter of the leader of the Neustrian aristocracy, the *Maior domus* Waratto.

Finally, a close reading of the LHF suggests that Charles Martel's victory over his father's widow, Plectrude, was made possible through the support he received from his wife's kin. Gerberding disputes the hypothesis that the area around Liège had long been in Carolingian control, suggesting rather that it was in this region that Charles was able to enlist his wife's family in his dramatic defeat of the other branch of his family.

In arguing the value of the LHF, Gerberding demonstrates a fine knowledge of the literature, a commendable ability to work with diplomatic as well as narrative sources, and a willingness to take on the big names both living and dead in Merovingian history. Some of his arguments parallel those by other scholars whose works have appeared since the completion of his manuscript.

Not all of Gerberding's hypotheses are equally convincing. At times some of his logic appears circular, and he uses the argument *ex silentio* more than one would wish. But, in the Merovingian game, universal agreement will never occur. We welcome a new and talented player to the field.

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CHARLES M. RADDING. *The Origins of Medieval Jurisprudence: Pavia and Bologna, 850–1150*. New Haven: Yale University Press. 1988. Pp. xiii, 258. \$25.00.

For more than a century, immensely learned, abstruse debates have raged about the medieval rebirth of Roman law and the origins of the jurisprudence that flowered at Bologna in the eleventh century, ultimately producing two competing schools of interpretation. One approach suggests that the rebirth of legal culture was the result of political or social changes wholly external to the law itself. Scholars who employ this approach have variously identified events such as the political furor of the investiture controversy or the social and economic revival of the northern Italian city-states as the underlying cause of the renaissance of the law. The other school of thought has identified the crucial factor behind the re-birth of jurisprudence as a massive borrowing by Bolognese jurists from the enormous corpus of Roman legal science that Justinian's lawyers excerpted and collected in the Digest. According to scholars from this school, the *studium* at Bologna became the great European center of legal education in the second half of the Middle Ages because Irnerius and his successors rediscovered the Digest and began to apply the abstract reasoning that had been characteristic of classical Roman jurisprudence and was almost totally lacking in early medieval law.

Charles M. Radding challenges the conventional orthodoxies and proposes an alternative theory that focuses on the judges tied to the capital of the Lombard kingdom at Pavia. Radding suggests that it was these *iudices sacri palatii* who took the decisive first steps away from the unreflective application of codified rules that had been characteristic of early medieval adjudication and began instead to engage in the kind of persuasive discourse that produces reasoned opinions concerning the application of the law to specific factual situations. According to Radding's vision, the Pavese judges, like other judges in early medieval Europe, had no need for principled explanations of their judicial decisions, so long as they all emerged from the same locale and the same professional training at

Pavia. Only after the royal palace ceased to function as the exclusive hub of judicial training and practice, in the last quarter of the tenth century, did the judges begin to approach cases from significantly different perspectives of training and experience. Hence, after 1000, the dispersed, diverse judges who came together to decide cases at Pavia no longer shared common training and assumptions, so they had to explain their legal decisions by articulating reasons for what they did. Radding suggests that it was only at this moment, when the judges began to experience dissonance among themselves, that the abstract principles of Roman jurisprudence became useful, and Radding goes on to argue that the Lombard jurists did begin to adopt the methods and the vocabulary of the Digest. Hence, Pepo and Irnerius appear in Radding's account not as the inventors of a new jurisprudence based on Roman law but as the heirs of the Lombard jurists Guglielmo and Bonifiglio.

The ambitious and controversial argument developed in this book rests upon broad inferences drawn from slender evidence and vague theories of social psychology. Radding purports to explain the rebirth of jurisprudence in terms of people rather than books, but in fact he has discovered virtually nothing about the Lombard jurists, their lives, or the milieu in which they worked. Data about the people in this book consist of the names of the Pavese judges arranged by the dates of the pleadings that the judges witnessed. The theoretical support for Radding's central argument is never expressed, but it comes down to two highly debatable propositions: that primitive legal cultures produce no reasoned legal discourse because the perceptions and values of people in primitive societies are uniform and that the sudden appearance of dissonant perceptions and values is the sole cause underlying the emergence of rationalized explanations for legal outcomes. These assumptions reflect neither an accurate grasp of the complexities and tensions that underlie the apparent unanimity and uniformity of primitive societies nor a willingness to address the fact that in early medieval settings, such as the polyglot realms of Clovis and Charlemagne, clerical and lay judges came together from diverse backgrounds and nevertheless failed to find it necessary to engage in juristic analysis as a means of explaining their decisions. Finally, in arguing that people, not books, explain legal evolution, Radding permits rhetoric to overcome logic. He attacks Stephan Kuttner's assertion that "it is unthinkable that a science of law could have taken shape in the medieval west without the rediscovery of Justinian's Digest." Radding claims that Kuttner's approach "place[s] books and not people at the

center of cultural development" (p. 115). Yet Radding himself carefully chronicles the introduction of the Digest into eleventh-century legal literature and points to the familiarity of Lombard jurists with the Digest as an important measure of the rebirth of jurisprudence, because Lombard jurisprudence prior to the reintroduction of the Digest fell short of systematic legal science. Radding has ignored what legal historians have long understood: books are important, too. Moreover, the best textual historians, such as Kuttner, have always taken scrupulous account of the human dimensions of medieval jurists insofar as there is credible evidence of those dimensions to be found.

Radding provides the best survey of Lombard jurisprudence available in English. His book is well-written, and he makes an argument that is bound to generate some interesting controversy. Yet ultimately most historians will continue to be persuaded by precisely the kind of careful, text-based scholarship that Radding eschews.

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THOMAS F. X. NOBLE and JOHN J. CONTRENI, editors. *Religion, Culture, and Society in the Early Middle Ages: Studies in Honor of Richard E. Sullivan*. (Studies in Medieval Culture, number 23.) Kalamazoo: Medieval Institute Publications of Western Michigan University. Pp. 256. Cloth \$32.95, paper \$15.95.

The reader will find little to quarrel with in this volume dedicated to Richard E. Sullivan. The editors have provided essays on intellectual, political, and social history by fourteen well-known historians. The collection opens with Archibald R. Lewis's article, which explains how Constantinople became the capital of the Roman empire. The active center of the Roman world moved from the Mediterranean to Constantinople, the Moselle Valley, and the Po-Adriatic region. Inhabitants of these areas could not very well communicate with each other, and new worlds were created around them by the Byzantines, the Carolingians, and the Venetians.

Next William M. Daly proves in a few pages, against prevailing views, that Sidonius Apollinaris became an intellectual Christian with his conversion. Then Donald Hochstetler demonstrates that the independence and the functions of St. Jean at Arles, the monastic community for women set up by Caesarius of Arles in the early sixth century, were dependent on its physical separation from "the outside world" (p. 27). Outsiders were excluded from this house of religion; only clerics necessary for the celebration of the mass were to

enter. Rule of absolute stability, which meant that the cloister retained the members, applied in the monastery. Only the abbess could eat by herself if she was ill, if the monastery lacked food, or if some business prevented her from eating with the sisters. There was trouble with the permanent alienation of ecclesiastical properties to the monastery; two popes allowed Caesarius only temporary transfer of ownership.

Political in tone is Jeremy du Quesnay Adams's essay. In wonderfully rhetorical language, he tests the degrees of similarity between Visigothic assemblies and medieval parliaments and concludes that, in particular, the Eighth Council possessed all of the characteristics of the Visigothic institutions (p. 653). Only the control of the fisc was very vague, and this may be attributed to high standards of ecclesiastical comportment or to careful control by the king over the councils. In parliaments of the fourteenth and fifteenth centuries, money matters became the main business, but English parliaments of the twelfth and thirteenth centuries and the Spanish Cortes dealt with claims similar to those of the Visigothic councils.

A worthwhile effort is Walter Goffart's undertaking to bring order into the manuscript tradition of Gregory of Tours. Goffart argues against the view and its variations that Gregory wrote two versions of his famous text, the unabridged *Histories* and a shorter *History of the Franks*; only the *Histories*, Goffart contends, were written by Gregory. Subsequent versions, the B and also the C manuscripts, were written by later editors. Goffart also argues that the Latinity of the *Histories* needs to be reworked; it reveals a more classical author than the Merovingian style of version B has let us suppose.

Kathleen Mitchell attempts to reconcile the hagiographic materials by Gregory of Tours with the interventions of St. Martin of Tours as a social healer of illnesses in the Christian body politic. She shows that Gregory followed in St. Martin's footsteps. Both protected the kingdom "by raising fears of retaliation in the guilty" (p. 79), not forgiving ambition, violation, and thefts by them.

Two essays concern an iconoclastic debate. Thomas F. X. Noble treats the major contributions of St. John Damascene in changing the image controversy to one of the theory of salvation, making Christ both the divine logos and man, a metaphysical person. Damascene's name is hardly mentioned in the subsequent councils because he attacked the imperial interference in dogma. David F. Sefton considers the papal pronouncements regarding the investiture strife, which have been dismissed by other historians as insignificant or political. After citing several other statements, he refers to Pope Hadrian's letter to the Council of

Nicea in 787, its mistranslation of a word in Latin, the sending of this letter to the Franks and their objections to it, and Hadrian's reply. All of this evidence leaves no doubt that papal judgments on this matter were similar to John Damascene's view.

The meaning of the Merovingian and the Carolingian kingship is the subject of David Harry Miller's article. He is right in stating that the conversion of Clovis did not result in the adoption of Christianity by the Frankish people, but his proposition that Gregory of Tours's accounts of Clovis's battlefield conversion as well as the influence on him by his wife "must be dismissed as pious frauds" (p. 135) needs correction. Clovis could have had imperial consent to his rule by becoming an Arian. The proposal that the papal pronouncement made Pepin's throne worthy but that he was rendered king through "election and elevation by public assembly" needs further study.

An excellent description of what distinguished a Greek polis from an early Carolingian parish is provided by John J. Contreni. Royal documents and episcopal statutes defined the centrality of the parishes that were identified as political, religious, social, and economic focal points, where education and legal documents could be obtained from the parish clergy.

Thomas L. Amos traces the factors that made monks become priests in the early church. Foremost were the Irish monks who came to the Continent. Then St. Benedict's *Rule* and St. Gregory the Great extended the pastoral work of the monks. The Anglo-Saxon monk-priest missionaries sought cooperation with the clergy. The vain official effort to remove the monks from pastoral service occurred under Charlemagne. But the monk-pastors were more literate. They preached and continued to serve in the numerous parishes controlled by the monasteries.

The linguistic differences that linked godparent and godchild, baptizer and baptized, cofathers and comothers, and spiritual siblings in the early Middle Ages is Joseph H. Lynch's undertaking. Bernard S. Bachrach examines how many times Fulk Nerra, count of the Angevins (987–1040), went on pilgrimages to the Holy Land and whether he went to gain absolution for the murders he committed.

The book ends with Karl F. Morrison's reappraisal of the *Dialogues* (1149–50) of Anselm of Havelberg. Previous historians have attributed an optimism to Anselm because, despite his conviction of Roman primacy, they thought he believed in church and state rapprochement. Morrison, on the contrary, is of the opinion that Anselm was a pessimist, although Anselm gave credence to the view that his polemics would bring the two adversaries closer through the powers of love.

Despite ideological differences among the essayists, I would recommend this book to all medievalists, particularly specialists of early Western history.

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SUSAN MOSHER STUARD, editor. *Women in Medieval History and Historiography*. (Middle Ages Series.) Philadelphia: University of Pennsylvania Press. 1987. Pp. xvi, 203. \$26.95.

This anthology of essays inquires into the treatment by historians of medieval women from their own day to the present. There is a bibliography, an index of subjects, and an index of proper names. The contributors concentrate on the traditions of historical writing of England, France, Germany, Italy, and North America.

The essays seek to find out whether a history of women in the Middle Ages was ever written, and, if so, why it has been neglected. The answer to the second part of this question varies according to the particular concerns of a nation's historians. We must try to understand historians to appreciate the way in which we recover knowledge about the past.

In "History of Medieval English Women," Barbara A. Hanawalt reviews the position of women in English historical writings. Late twentieth-century scholars tend to investigate medieval women less for polemical reasons than for general interest in their lives. Such interest is linked with the study of social history and with political developments in support of women's rights. The 1860s and 1960s witnessed research into the status of women in the Middle Ages and in history in general. The essayist maintains that the search for women's rights in the nineteenth and twentieth centuries gave rise to "two golden ages in the history of medieval women" (p. 18). There was a brief lull in the 1920s when the women's suffrage movement was thought to have achieved everything that women wanted, but impetus was soon regained. As political history has broadened from a narrative of battles and an analysis of constitutional and administrative changes, women have gained greater prominence.

Diane Owen Hughes in "Invisible Madonnas? The Italian Historiographical Tradition and the Women of Medieval Italy" looks at work written in Italian city-states as well as that of the Risorgimento. She argues that women's key role in the private domestic world was seen as an obstacle to their having any role in the public world of history. The division between the public and private

worlds is responsible for the attitude adopted toward women in much Italian historical writing. The capacity of writers to account for women through the ages as saints, or at least repositories of virtue, both in life and in literature is alluded to by both Hughes and Susan Mosher Stuard in her essay, "Fashion's Captives: Medieval Women in French Historiography."

Stuard notes, however, that, although medieval French chronicles and histories treat women freely in public as well as in private roles, the historians of later centuries took a different approach, even though they paid attention to medieval sources. Romantic historians represented women as embodying abstract qualities. Such writers drew into their work women whose lives could be imitated or taken as a cautionary tale. They also paired women with male figures so that medieval women never appeared as autonomous persons who controlled events. The best historians escaped from such confines, although occasionally the drive to focus on women went wrong, as in Louise Keralio Robert's *Les crimes des reines de France* (1791). This work produced a clutch of forceful and wicked queens to take hold of the popular imagination, doing little service to the study of women in history. Late nineteenth-century positivists dropped women from historical texts, for their activities rarely belonged to public, verifiable history. Women's names and acts were recorded in editing medieval texts, especially those related to economic and social history, but such information about women was rarely interpreted. France appeared to have emerged as a great nation "without Frenchwomen playing any consequential public roles whatsoever" (p. 68).

A revolution occurred with the rise in importance of social and economic history. Women had to loom large in the history of the masses and of the family. At first women were overlooked because historians still relied too closely on positivist models. The mistake was made, too, of expecting medieval women to be limited to the same spheres of influence as women of later centuries, so no dynamic study of women could take place. Only with women themselves taking part in writing and interpreting history could real changes in approach be noted.

A favorable view of women in Germanic historiography emerges in the chapter by Martha Howell with Suzanne Wemple and Denise Kaiser. Traditions of historical scholarship established in Germany and the Low Countries in the nineteenth century gave rise to some of the best studies of medieval women. Leopold von Ranke saw the historian's job as "to understand the past on its own terms" (p. 104), studying the surviving documents, the conditions in which they were pro-

duced, and their creators. From this method developed a history of medieval women based on an examination of original documents, a development made more possible here than elsewhere by the rise in interest in social and economic history. A general romantic interest in Germany's medieval past fostered an interest in medieval women, and feminist action in the late nineteenth and early twentieth centuries also affected German historians. Studies of legal systems and marriage customs were very important and influential, for there was "abundant documentation left by medieval urban institutions recording inheritances, marriages, guardianship practices and property relations" (p. 115). The activities of medieval women in the marketplace and in the economy in general were studied, concluding at least once that it was dangerous to permit women to move away from the household, which ought to be at the center of their lives. Studies were made of female rulers, female religion, witchcraft, prostitution, and gynecology. Such pursuits lost impetus during the Nazi period and did not immediately revive after the war. Although the authors find disheartening the reluctance of German medievalists to take account of work being done elsewhere, overall the debt owed to German scholarship of medieval women is great.

In total, though not an "easy read," this book makes sense of several approaches to the history of medieval women, and, because of its copious references, it is a useful starting point for anyone wishing to delve into the subject.

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CAROLINE WALKER BYNUM. *Holy Feast and Holy Fast: The Religious Significance of Food to Medieval Women*. (The New Historicism: Studies in Cultural Poetics.) Berkeley and Los Angeles: University of California Press. 1987. Pp. xvi, 444.

During the last supper, Christ took some bread, turned to his disciples, and said "Take, eat; this is my body." He then took a cup of wine and also gave it to them, saying "For this is my blood of the covenant, which is poured out for many for the forgiveness of sins" (Matt. 26: 26-28). For centuries this scene has been symbolically reenacted, albeit with a variety of interpretations, in the central ritual of the Christian liturgy. Food, in its multiplicity of meanings, thus is located at the very core of Christianity. Yet, until Caroline Walker Bynum's study, the importance of food for religious life has not been adequately perceived. Revealing perhaps more about modern concerns

than about the centuries that separate our own time from the birth of Christianity, scholars have tended to focus on chastity and poverty—sex and money—as the central features of medieval asceticism. Bynum's brilliant new book redresses the balance by looking at the religious significance of food to late medieval women.

The author places her analysis of the link between women's religiosity and food within a broad cultural context. She traces the roots of the most important food practices of medieval Christians (the fast and eucharistic devotion) and examines the religious options open to medieval women, the ways in which religious women used food for the social manipulation of family and community, and, finally, the meaning of food as a religious symbol.

Although food was important to the religious experience of both men and women, it was particularly crucial to women's spirituality because it was the one social resource over which they had control. For women the renunciation of food was the analogue to the renunciation of wealth and of power among men. The late Middle Ages therefore saw the proliferation of food-centered religious practices among women, most notably food miracles, eucharistic piety, and fasting.

But what is one to make of the kinds of ascetic practices presented in this book? The author details the stories of female saints and mystics who denied themselves all nourishment except for the eucharist, who ate but were unable to digest their food, and who sought religious fulfillment through the ingestion of fares (one hesitates to call them food) that would turn the strongest stomach. Such tales have revolted and fascinated modern scholars, whose interpretations have taken on a variety of negative shadings. Conservative historians of theology have charged the women with misguided religious zeal. Some feminist scholars have argued that they were victims of the patriarchal society in which they lived and that their asceticism was a form of internalized misogyny. Yet other historians have emphasized the medical and psychiatric aspects of food asceticism by calling it anorexia.

In a thrilling and undoubtedly controversial reinterpretation of the evidence, Bynum argues instead that quite irrespective of how contemporary historians view these practices, as seen in their medieval milieu, they were expressions neither of self-hatred nor of world-denying dualism. Through denial of food, medieval women not only controlled their own bodies but controlled their environments. In a technologically primitive world where famine was never too far removed, food asceticism linked to charity literally fed those who were on the brink of starvation. "Let the

abstinence of the faithful become the nourishment of the poor," ordered Pope Leo the Great, "and let the indigent receive that which others give up." More important, however, from a religious perspective, was the belief that by undergoing fleshly suffering female mystics fused their own humanity with that of Christ, whose human suffering allowed redemption. Rather than try to rise to the level of spirit, and thereby become symbolically male, religious women reveled in their own humanity. By denying themselves food and experiencing the sufferings of Christ, they fed on God and became food for the spiritual nourishment of those around them.

In an ironic twist, Bynum also claims that women's subservient status in society made it easier in some ways for them to imitate Christ and embrace him in mystical union, whereas men on the same quest had to experience symbolic reversals that stripped them of the power and status they had in society. Bynum rests this part of her argument on diverse but mutually reinforcing types of evidence, ranging from the writings of medieval mystics to feminist psychoanalytic theories that stress that in Western society dichotomy and reversal have been constant features of the psychological development of male children, who must learn that they cannot be the mother on whom they first projected themselves. Given the importance of these theories to Bynum's argument, one wonders why she does not elaborate further on the family life of the mystics she studies. Female mortality rates and child-rearing customs must often have separated female children from their mothers or female caretakers at a tender young age. Did such childhood experiences affect the meaning of religious symbols for some female mystics?

Another question that emerges from Bynum's work is why food-centered religious practices took on particular importance for women in the late Middle Ages. Bynum is persuasive in her claim that from the late twelfth to the early fourteenth centuries there were growing opportunities for women to participate in new religious roles at the same time that there was a decline in their quasi-clerical roles. Yet the handling, preparation, and distribution of food had always been a function of women. Why had they not expressed their spirituality as strongly in food-related practices in the early medieval period?

These questions are not meant to detract from this bold and rich reinterpretation of medieval spirituality. A brief review can hardly do justice to the subtlety of Bynum's argument, her imaginative use of difficult sources, and the breadth of her vision. Bynum forces us to rethink our most fundamental assumptions about the meaning of reli-

gious beliefs and practices, about women's roles in Western society, both past and present, and about how human beings construct their lives. There are few works of medieval history or feminist scholarship that can claim to be its equal. This is a masterpiece.

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WOLFGANG STÜRNER, *Peccatum und potestas: Der Sündenfall und die Entstehung der herrscherlichen Gewalt im mittelalterlichen Staatsdenken*. (Beiträge zur Geschichte und Quellenkunde des Mittelalters, number 11.) Sigmaringen: Jan Thorbecke. 1987. Pp. 276. DM 80.

At the Diet of Roncaglia in November 1158, Ubertus, the archbishop of Milan, responded to Frederick Barbarossa's opening address with a eulogy of Frederick. In it he described the origins of all authority. The first man had dominion over the fish of the sea and the fowl of the air (Genesis 1.28). Now, for our sins, man rules over man ("Peccatis exigentibus homo homini dominatur"). After a long line of kings or, rather, tyrants, the archbishop continued, Italy was blessed with Frederick's rule. Some seventy-five years later, Frederick's grandson issued the Constitutions of Melfi and wrote in the prologue that God bestowed authority on the princes of nations to correct the crimes of God's creatures and to protect them. Sin was a powerful idea in Christian thought. It directed the destinies of believers in the next world and affected their lives in this. Wolfgang Stürner has explored the origins of these ideas and their development in later thought.

The range of this book is impressive. Stürner traces these concepts from the early fathers of the church to Luther. At the end of the second century, Irenaeus of Lyon was the first to connect sin and authority. He was inspired by Paul's Letter to the Romans 13.1-2. Paul had written that every soul must be subject to its superiors and that all authority is established by God. Irenaeus took Paul's admonition one step further: God established rulers because, when men were left to themselves, they committed murder and were overcome by greed. Stürner is a historical pointilist, and there are very few texts that he has overlooked in his account of how Irenaeus's invention dominated theological and political thought until the middle of the thirteenth century. At that time, Aristotle's view that authority was a natural result of human concourse took its place alongside older views. St. Thomas Aquinas was the first theologian to integrate Aristotle into his thought, and he understood power and authority in this

world as being a natural human need. But this new theory did not sweep the field. The new was mixed with the old in varying proportions. And Marsilius of Padua, Dante, John of Paris, and other later thinkers reflect this change in varying degrees. The high papalists of the late Middle Ages probably gave this web of ideas its most ingenious shape. They argued that, since all power came from God to rein in the malevolence of man, God's ordained institution on earth, the church, must mediate the bestowal of this authority on rulers.

Stürner has written a learned and intelligent book. I would have put more emphasis than he has on Roman law and its doctrine that authority could be derived from the people, but that may reflect my prejudices and may not be a balanced view of the origins of political authority in Western thought. This book will now be the standard work on those ideas.

KENNETH PENNINGTON
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PETER RAEDTS, *Richard Rufus of Cornwall and the Tradition of Oxford Theology*. (Oxford Historical Monographs.) New York: Clarendon Press of Oxford University Press. 1987. Pp. xiii, 272. \$53.00.

The notion of a distinctive Franciscan school of theology at Oxford in the thirteenth century goes back among English scholars to Andrew G. Little and Dorothea E. Sharp, who agreed that the doctrinal traits of this school were set by the thought of its presumptive founder, Robert Grosseteste, not himself a Franciscan. Since recent years have seen a minor renaissance in studies of Grosseteste, it is not surprising that the idea of an Oxford Franciscan school has also come under review, its characterization even under attack. Richard Southern fired the first salvo with his *Robert Grosseteste: The Growth of an English Mind in Medieval Europe* (1986), arguing that Grosseteste was more insular than had been suggested. Now Peter Raedts, a student of Southern, has gone a step further by taking aim at one of the early Oxford Franciscans, Richard Rufus of Cornwall.

Raedts argues that there was a profound cleavage between the theological methods and attitudes of Grosseteste and Rufus and that Rufus, not Grosseteste, was indicative of the way Franciscans at Oxford thought about theology in the years leading up to Duns Scotus. Raedts further insists that this cleavage reflects a monumental transformation in the nature of medieval thought, a move toward a world view in which the sacred was disentangled from the profane, grace more clearly separated from nature, and human autonomy more precisely and securely defined. On all of

these matters Grosseteste was old-fashioned, while Rufus and subsequent Oxford Franciscans looked ahead, preparing the way for the insights of Scotus and even William of Ockham in the years to come. This also means, according to Raedts, that Oxford Franciscans, unlike Grosseteste, were in tune with the developments of the continental scholasticism of their day.

The first half of Raedts's book concentrates on establishing what we can know with certainty about Rufus's life and determining the canon of his authentic works. Awkwardness of language and a rambling style make the going rough, but the arguments here are persuasive. In the second half, where Raedts turns to his broader historical theme, the generality of his conclusions sometimes strains the resources of the evidence brought to bear. Grosseteste and Rufus surely differed on whether commenting on the *Sentences* was proper work for a theologian and even whether theology might formally be made into a science, as Raedts claims in chapter 6, but it is not as clear that they disagreed about the value of dialectics and logical argumentation. The material surveyed in chapter 7, on God's work of creation, does not reveal the firm distinction between Grosseteste as a participationist and Rufus as a worldly Aristotelian that Raedts wants it to.

Yet, if one can quibble about specific arguments, the general tenor of Raedts's interpretation rings true. The tight analyses in chapters 8 and 9, on free will and the incarnation, make a more persuasive case for Rufus's novel interest in logic and his conscious rejection of a participationist metaphysics than the much longer chapters that come before. The chapter on the incarnation brings home to the reader what it meant for Rufus to make the break between sacred and profane that Raedts has in mind. Raedts was right to think that he could find matters of general importance in a close study of the thought of a secondary figure like Rufus. His efforts add considerably to our understanding of scholasticism in its critical years at mid-thirteenth century.

STEVEN P. MARRONE
Tufts University

WILLIAM J. COURTENAY. *Schools and Scholars in Fourteenth-Century England*. Princeton: Princeton University Press. 1987. Pp. xix, 435. \$48.00.

Influenced largely by the work of French scholars, historians in the 1950s began to reinterpret medieval scholasticism by integrating it into its institutional context. Taking scholasticism at its root meaning, they saw it not exclusively as cerebral speculation but as an outgrowth of the pedagogy

of the schools and universities. Now that a later generation of historians has provided the indispensable groundwork, English schools and thought are ripe for comparable treatment. William J. Courtenay, who has made important contributions to these preliminary studies, offers us here a rich and persuasive synthesis of schools and scholars in fourteenth-century England. Beginning with the educational context, he plots the growth of elementary schools of reading and song, secondary schools of grammar, and the universities at Oxford and Cambridge, which followed the lead of Paris. He shows how students drawn to the mendicant *studia*, cathedral schools, and the two universities were subsidized by the church and the king for the expressed purpose of recruitment into ecclesiastical and royal administration as well as for the expanding services of the religious orders.

Within this institutional context Courtenay detects the decline of the vast philosophical-theological systems of Thomism, Aegidianism, and Scotism inherited from the Continent in the thirteenth century. (The one exception was a perennial revival of Augustinianism.) In their place appeared a more precise *logica anglicana* of terminist logic, which explored the philosophical implications of language; a heightened interest in a physics that employed mathematical and axiomatic models; and a more nuanced *theologia anglicana*, which applied the new logic to a more selective range of theological questions. Led by scholars at Oxford and the Franciscans, England declared its intellectual independence from the Continent and challenged Paris's supremacy with the scholarship of theorists of the stature of William Ockham and Thomas Bradwardine.

This momentum slackened by the 1360s, but not apparently as a result of the Black Death or of the Hundred Years' War, because the intellectual recession antedated both catastrophes by at least a decade. For reasons that are not yet entirely clear, English intellectual life not only declined but also shifted in the second half of the fourteenth century from the Latin schools of Oxford to the vernacular courts of London. Although Courtenay's treatment brilliantly illuminates the world of Ockham, it does not yet fully prepare us for that of Chaucer. Within this survey Courtenay nonetheless provides a masterful distillation of the philosophical and theological thought of Ockham, a jargon-free guide to the subtleties of terminist logic, and a comprehensive catalogue of practicing theologians and their treatises, as well as a lucid orientation to the English intellectual scene.

The fruitfulness of this institutional approach can be seen in Courtenay's ability to make sense of important intellectual shifts in the fourteenth cen-

ture. For example, the universities had become so successfully integrated into society that academic training became increasingly requisite for the highest careers in the church and government. Because of these outside attractions, fewer scholars entered into their studies with the goal of remaining teachers. The result was the shortening of university careers to between ten and twenty years. Most masters had left by the time they entered their forties, and Ockham, for example, composed his major works within the brief period 1317–26. The promise of future rewards, moreover, drew increasing numbers to the universities. Because a regency (the period of officially authorized teaching) was still required for all those seeking the master's degree, the regencies were shortened and rotated quickly to accommodate the number who sought them. In place of the regent masters, more instruction was performed by teaching bachelors. The rapid turnover discouraged the arduous *questiones disputatae* and comprehensive theological *summae* in favor of shorter treatises and narrower definitions of problems. Decreasing continuity within the teaching personnel promoted the disappearance of comprehensive systems of thought, which were not, in fact, replaced by a "school" of nominalism. Metaphysical questions of "why" acceded to a logical and methodological approach of "how." In short, the waning of medieval scholasticism, itself the fruit of the medieval schools, can be accounted for, in part, by the social success of the universities.

JOHN W. BALDWIN
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DAVID H. WILLIAMS. *The Welsh Cistercians*. In two volumes. Tenby, United Kingdom: Cyhoeddiadau Sistersiaidd. 1983. Pp. xiii, 195; 198–372. £16.

The author of many articles on the Welsh Cistercians has brought his studies together here in this broad and valuable synthesis. Although a traditional work with no new approach or interpretation, it is based on deep and careful investigation of many types of evidence, including the archaeological, and is written by an obvious authority.

In section A of volume 1 David H. Williams sketches the genealogical filiation and the chronological history of the Welsh houses from the foundation of Neath (1130), Tintern (1131), and Whitland (1140) to the suppression of the abbeys by Henry VIII and Cromwell in 1536–39. In section B the author treats some aspects of community life, such as conventual buildings, personnel (choir monks and conversi), the lay communities often attached to the monasteries, and relations with

the "international" Cistercian Order through the general chapters and the system of visitation. Volume 2 is devoted almost entirely to the abbeys' economic activities. In an awkward arrangement, both volumes contain appendixes, although the indexes are all in volume 2. Appendix 1, which lists all known superiors of the fourteen houses of men and two of women, suggests the need for the revision of David Knowles's *Heads of Religious Houses in England and Wales, 940–1216* (1972). Appendix 2 shows the architectural plans of many of the abbeys, and appendix 3 (in volume 2) is a comprehensive table of the relative valuation of the monasteries between 1291 and 1536–39. The indexes provide examples of charters, abbatial seals, and illustrations of nineteenth- and twentieth-century sketches of monastic ruins.

Scholars will probably find Williams's studies of the Welsh Cistercians' economic activities of greatest interest. His conclusions support the pattern of White Monk operations elsewhere: many grants of land to the monks, described in the charters of foundation or endowment as gifts, were actually rents in disguise; Cistercian acquisitiveness for the consolidation of scattered estates led to the displacement of villages and settlements and brought on them considerable criticism; the Cistercians in Wales took advantage of the economic opportunities available in their varied localities in sheep farming, fish hatcheries, horse breeding, coal, lead, and iron mining, stone quarrying, as well as in land reclamation. Manorial rights brought broad legal jurisdiction and yielded customary income through mills, markets, fairs, fines, amercement, and outfangthief. On the other hand, the well-known banking activities of such abbeys as Dore receive no mention.

Although an impressionistic attempt is made to treat the relations of patrons to the abbeys they founded and endowed and the social origins of the monks, the social historian will look in vain for the kind of sustained analysis that appears in the studies by Constance Brittain Bouchard on the Cistercians of Burgundy. Further, Williams is not familiar with the theories of Constance Berman on the recruitment of the conversi in southern France. The two houses of nuns, Llanllugan and Llanllyr, receive only the scantiest of notice. Williams's prose is very condensed and sometimes labored.

These caveats aside, students of Welsh Cistercian history will have to consult this book, and an atlas of Cistercian lands in Wales is promised for the near future.

BENNETT D. HILL
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ANDREAS MEYER. *Zürich und Rom: Ordentliche Kollatur und päpstliche Provisionen am Frau- und Grossmünster 1316–1523*. (Bibliothek des Deutschen Historischen Instituts in Rom, number 64.) Tübingen: Max Niemeyer. 1986. Pp. xi, 625. DM 162.

This is one of the most precise and detailed studies yet published on the actual workings of the benefice system, the great pork barrel of the late medieval church. Andreas Meyer's work is particularly admirable because he spans with equal mastery the realms of legal and social history, which all too often go their separate ways. In this book, completed as a dissertation at Zurich in 1984 under the supervision of Ludwig Schmugge and grounded solidly in sources in the Vatican Archive as well as in Switzerland, Meyer analyzes the disposition of the more than eighty major benefices at the two most important churches in Zurich in the two centuries before the Reformation. The Grossmünster, according to tradition founded by Charlemagne, was made immemorial by Zwingli, but the Fraumünster historically played a no less significant role in the life of the city. Founded in the ninth century by Louis the German as a house of Benedictine nuns, its abbess was originally lord of Zurich, although in the late Middle Ages she fell increasingly under the thumb of the city government. As a font of male ecclesiastical patronage, the Fraumünster took on importance from the twelfth century as the chapter came to include secular clerics who were attached to the Fraumünster and whose numbers by the late Middle Ages outstripped those of the nuns.

The narrative section of the book (173 pages) includes a discussion of the sources, an excellent exposition of the intricacies of the system of papal provisions, and a multiaxial analysis of the disposition of benefices divided according to pontificate and mode of collation. The book is illustrated with twenty tables and is replete with percentages. The great bulk of the work is prosopographical (pp. 175–522), consisting of all ascertainable relevant biographical data on the 1,053 clerics who, during the years 1316–1523, sought to obtain one of the benefices at either church. Meyer supplies information on their ecclesiastical careers elsewhere as well, so that these biographies have more than local interest as rich lodes of information. Among Meyer's many findings several are worth noting. Of these 1,053 clerics, 219 failed to secure a benefice at either church. The 1,093 recorded institutions of benefices went, then, to 834 clerics, who had an average incumbency of eighteen years. The legal basis of these installations, however, is determinable in only 324 cases or 29.7 percent of the total. Meyer confirms the accepted

view that expectancies provided a much greater likelihood of success than did provisions. The picture he composes of the ups and downs in the competition among patrons and among impletrants also conforms to the pattern evident in much of the rest of Europe or, at least, of German Europe. Specifically, for much of the fourteenth and fifteenth centuries, university men, the principal beneficiaries of papal provisions, advanced at the expense of the local urban aristocracy in obtaining canonries. By the later fifteenth century, however, the scions of the patrician families of Zurich again dominated these chapters. At the Grossmünster their share of the canonries rose from a mere 12.5 percent in 1440 to 41.6 percent in 1480 to 66.6 percent in 1500 and 1523. The crucial turning point here was Pope Sixtus IV's delegation to the city councils of Zurich in July 1479 of the right to dispose of benefices during the papal months. This was but another step in the crumbling of the papal monarchy, and in the 1520s the government of Zurich, like so many others in Europe, completed the displacement of the papacy in the running of the church.

To one minor conclusion I would raise an objection. Meyer believes that episcopal rights of collation here suffered a considerable setback in the late Middle Ages. I am not persuaded of this either in these instances or as a general proposition, and, to the extent that these losses occurred, they did so in the fourteenth rather than in the fifteenth century. But this is a minor disagreement, for the bishops of Constance, like their confreres elsewhere, were little more than onlookers in the great struggle over ecclesiastical patronage.

LAWRENCE G. DUGGAN
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GEORGE OVITT, JR. *The Restoration of Perfection: Labor and Technology in Medieval Culture*. New Brunswick, N.J.: Rutgers University Press. 1987. Pp. xiii, 272. \$32.00.

The study of medieval technology and its cultural implications has become an important part both of medieval studies and of the current debate over the origins, development, and impact of modern (Western) attitudes toward nature, work, and technological advance. Historians as well as philosophers, theologians, and environmentalists have looked to the Middle Ages for clues to explain the unique growth of technology in the West and to explain the origins of Western perceptions of technological development as variously progressive, destructive, or a mixture of both. George Ovitt's book is a valuable and much-needed effort

to sort out some of these complex and still largely unresolved questions.

The focus of the book is on the development of ideas and cultural attitudes; Ovitt, however, does not neglect the social context of medieval technology. One of the most useful sections comprises two introductory chapters clarifying some of the theoretical and historiographical issues associated with the history of technology and the history of ideas of progress in general. A final chapter synthesizes current scholarship on the social background of the work of artisans and peasants, with particular attention to women.

The main thrust of Ovitt's argument, however, is to take issue with the view, publicized if not wholly framed by Lynn White, Jr., that medieval Latin Christianity is the source of a still powerful ideology sanctioning the domination and control of nature for human benefit and defining manual labor as both morally virtuous and central to the pursuit of spirituality. Ovitt systematically and rigorously reviews the hexaemeral literature, monastic rules, treatises on the mechanical arts, and other theological texts, and concludes that "the attitude of the Church toward manual labor and its products was neither univocal nor even consistent during the Middle Ages" (p. 199), but rather fluctuated between the belief in labor as redemptive and an equally strong view that the *opus manuum* was properly subordinate to the *opus Dei*. Ovitt, moreover, suggests that by the end of the twelfth century the church had abandoned its earlier commitment to the monastic ideal of communal, cooperative labor and relegated labor to the increasingly separate world of secular life.

Ovitt's argument that medieval Christianity held an ambivalent attitude toward manual labor and technology is persuasive. It is not surprising that the intellectual tradition, rooted in classical and Christian dualism, should see technology as a double-edged sword or that medieval churchmen should be snobbish about working with their hands. Yet the book raises some questions that require further clarification. What, for example, is the meaning of the "secularization of labor" (Ovitt's term for the church's relinquishment of the earlier monastic ideal of communal, cooperative labor) when artisans and craftsmen themselves continued to justify labor in religious terms into the fourteenth century, as Ovitt himself shows? Again, although the book claims to deal with both labor and technology, the focus is much more on the former and in fact blurs a crucial distinction between the two: technology, unlike physical work, had at least the potential for intellectual respectability because it involves the brain as well as the hands.

The balanced and careful analysis of texts, am-

ply quoted, makes this book a welcome addition to the field. It is well written, well edited, and handsomely produced.

ELSPETH WHITNEY
Alfred University

JOSIAH C. RUSSELL. *Medieval Demography*. Foreword by DAVID HERLIHY. (AMS Studies in the Middle Ages, number 12.) New York: AMS. 1987. Pp. x, 325. \$37.50.

When reviewing this collection of Josiah C. Russell's papers, it is necessary to avoid expressing wonder simply at his sustained vigor and productivity. It also necessary to avoid the obvious irony of noting that a father of medieval demographic history has lived to such an advanced age.

Josiah Cox Russell is the doyen of active medievalists in the United States; he was born in the year of the century, and he is the surviving contributor to the 1929 *Festschrift* presented to Charles Homer Haskins. This volume of his studies is the second that has been produced, as a service to the scholarly community, by the publisher. *Twelfth-Century Studies* (1978) contained fifteen essays, written between 1932 and 1975. *Medieval Demography* has nineteen papers, first published between 1930 and 1983, plus a new introductory essay ("The Mythologizing of History"), an instructive preface by David Herlihy, and a Russell bibliography (which runs from 1927 through his 1985 "Demographic Factors of the Crusades").

The papers printed here fall into several main categories, and a few odd pieces stand by themselves. We can see that Russell returned, over the years, to favorite problems, especially those related to his demographic interests, and that he kept his zest for the odd offering, the feisty short paper that pokes at accepted views and tests our methodological strengths and weaknesses. He has a flair for taking the possible or the probable and then extending the argument until the logical boundaries of the case have been fully stretched.

Russell the demographer is the best-known Russell. His books on the subject (1948, 1958, 1985) are among the starting points of most discussions. Accordingly, we welcome the appearance here of six papers devoted to this area. The impact of the early medieval plague was a minor topic until Russell began to worry about it, and we can see its effects in Egypt and on the Continent. He also was an innovator in the use of diverse sources; an older orthodoxy held that medieval demography was a contradiction in terms because of the lack of usable sources. Russell shows that almost anything we can count is grist for this mill: bones in ceme-

teries, houses and hearths, clerics, names, parish boundaries, the incidence of coinage, and so forth. He turns his light on many corners of medieval Europe, and he ranges in time from the late classical period to the late fourteenth and early fifteenth centuries.

Before he broke ground as a demographer, Russell compiled the *Dictionary of Writers of Thirteenth-Century England* (1936). An interest in biography also led to reflections on the methodology of such reconstruction and on how a mythology was quickly generated. A long paper ("An Introduction to the Study of Medieval Biography" [1943]) and some shorter ones that examine the myth-history of Peter of Pontefract, St. Edmund of Abingdon, and the pseudo Edward II are in this vein. A few other themes appear. Some argumentative papers on Anglo-Saxon history are still of interest. How many of us realized that Russell had once been interested in the origins of Parliament, and that he wrote within the canons of the Whig tradition?

Reading these papers is, in a professional sense, fun. That is perhaps tribute. The boldness of their author is obvious from the start. I have talked with J. C. Russell at many a medieval conference. His opinionated style is an inspiration to the generations who have profited from his labors and his kindly and insatiable curiosity.

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JOHN McDONALD and G. D. SNOOKS. *Domesday Economy: A New Approach to Anglo Norman History*. New York: Clarendon Press of Oxford University Press. 1986. Pp. xv, 240. \$45.00.

All branches of history have been influenced by statistical analysis in recent decades. Historians who have been less than comfortable with these new approaches have sought refuge in periods of history that are less statistically rich. This kind of sanctuary is becoming increasingly hard to find. In this study John McDonald and G. D. Snooks subject the Domesday Book of 1086 to the scrutiny of quantitative analysis.

The results are extremely interesting, doubtlessly provocative, and, to me, largely convincing. The first two chapters describe the Domesday Book and its historical context. The final three chapters, which constitute almost half the book, constitute a long and difficult appendix explaining the statistical techniques employed. The substantive historical material is encapsulated in chapters 3 through 6 and is summarized in chapter 7.

Chapter 3 outlines the conventional wisdom

about Domesday established almost a century ago. This takes the view that Domesday tax assessments were "artificial" in that they were arbitrarily determined by the new political regime of the Normans without regard to the economic worth of the properties on which they fell. Similarly, it was believed, there existed no strong link between the tax valuations and the incomes of the manors. The analytical basis of this interpretation is questioned, and, in the two following chapters, it is strongly challenged by means of econometric analysis.

The statistical analysis is confined to the counties of Essex and Wiltshire, but they provide a substantial sample of both lay and ecclesiastical manors, almost fourteen hundred in total. All the regression results reveal a strong relationship between tax assessment and capacity to pay, suggesting that, far from being an arbitrarily determined valuation, the assessments were derived from the economic worth of the manors themselves. The results also show that the tax was levied not simply on arable land but on the total resources of the manor, including plow teams, livestock, and peasants. It was a form of income tax and was regressive, favoring the more prosperous to whom the king looked for support. Contrary to F. W. Maitland's view that "everywhere we are baffled by the makebelieve of ancient finance" (p. 79), this analysis reveals a close relationship between annual valuation and resources. These results establish the credibility of the Domesday Book as a meaningful record of the eleventh-century economy.

The third substantive analytical chapter seeks to derive a production function for this economy. Given the restrictive assumptions embodied in such models, the results must be treated with some reserve. They are, nevertheless, interesting and plausible, depicting an economy capable of achieving output from a wide range of combinations of resources, in which output was increased as more resources or inputs were exploited, and which even generated modest economies of scale. Wiltshire was rather more oriented toward pastoral production than Essex, although arable land was dominant in both counties.

The substantive chapters and their stimulating conclusions make this a book worth recommending to a wide audience of historians. There is plenty for specialists to debate, and the authors raise numerous issues to which their statistical analysis can provide only a partial answer. The long statistical section can be avoided, and it could have been shorter and simpler. But the exciting conclusions should persuade any doubters that familiarity with basic statistical methods is rewarding, even essential, for historians today. It is to be hoped that the analysis of the other counties in

Domesday that the authors promise will appear in print before too long.

CLIVE H. LEE
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DIANA GREENWAY *et al.*, editors. *Tradition and Change: Essays in Honour of Marjorie Chibnall Presented by Her Friends on the Occasion of Her Seventieth Birthday*. New York: Cambridge University Press. 1985. Pp. xvi, 269.

Festschriften fulfill many purposes and present many challenges to contributors, editors, and readers. As both affectionate gifts and admiring tributes to distinguished scholars as teachers, role models, and friends, they mirror the recipients' achievements while highlighting the best efforts of their donors. Although individual contributions frequently win deserved accolades in scholarly footnotes, too often a reviewer observes that these works "would otherwise be unlikely to see publication under a single cover" (*AHR* 82 [1977]: 7). On occasion a festschrift has seemed too focused (*AHR* [1982]: 1066–67).

The volume presented to Marjorie Chibnall has surmounted these and other hazards of the genre to achieve an unusual degree of unity, coherence, and usefulness as an integrated portrait of the Anglo-Norman world of the eleventh, twelfth, and thirteenth centuries. Although framed by the Norman Conquest and the consequences of Magna Carta, the collection also incorporates cross-channel impacts on "tradition and change." The title, rather a large and shapeless umbrella, nevertheless supplies to contributors and readers alike a unified focus of attention and approach. Diana Greenway, Christopher Holdsworth, and Jane Sayers, as editors, have crafted a gift of elegance and grace.

Chibnall's own work ranges widely from the eleventh to the thirteenth centuries, including Anglo-Norman monastic landholding, historical writing, lay and ecclesiastical administration, and feudal society. Both veteran and beginning scholars will appreciate the record of her career given in the foreword and bibliography, especially the saga of her years of work on Orderic's history, the survey of her master works, and the glimpses into the particular problems and special strengths of an eminent woman scholar.

Like Chibnall's work, the essays in this volume encompass two themes: ecclesiastical and secular administrations of religious institutions. First, Christopher Brooke and Christopher Holdsworth argue respectively for Norman introduction of archdeacons into England and Orderic's disapproval of monastic innovations, that is, Cistercians.

Giles Constable documents the stormy saga of Baume Abbey's subjection to Cluny amidst power struggles between pope and emperor. Although the scene is Burgundy, its juxtaposition to Raymonde Foreville's essay suggests instructive parallels. Foreville discusses the gradual crystallization of papal standards for canonization of saints as a reaction to competing Canterbury and English royal efforts to canonize their own exemplars, as well as an assertion of papal powers against them.

Greenway scrutinizes standards of episcopal governance outlined in the *Institutio* of St. Osmund (1091); her close comparison with later texts argues for compilation of these standards in stages from 1160 to 1215. Christopher Cheney, analyzing two newly discovered Canterbury mortuary-roll fragments, questions whether such rolls circulated only to direct ecclesiastical affiliates: Canterbury, at least, enjoyed wider influence in both England and Normandy. David Luscombe traces the transmission of Denis the pseudo-Areopagite's works from the Continent to twelfth-century England.

One-sentence summaries naturally cannot reflect each essay's scholarly depth, but they can indicate connectedness and related issues. For example, Brooke's discussion lays a foundation for Greenway's; both cite documents, dated between 1089 and 1093, that suggest the crystallization of episcopal administration—during the Canterbury vacancy under William Rufus—and raise intriguing questions (pp. 2, 5, 9–15, 83, 85, 89, 90).

Ecclesiastical questions flow naturally into secular realms with R. Allen Brown's convincing argument for a pre-conquest Norman chancery and a reappraisal by new criteria of post-conquest English charters. Edmund King illuminates the activities of the second-generation cross-channel baron Waleran of Meulan as earl of Worcester, correcting earlier scholarship. Mary Cheney discovers a previously unknown decree of Henry II but presents only one unconvincing example of the use of that legal procedure by Henry I (p. 192). Jane Sayers supplies a new perspective on Englishmen in the Third Crusade. Finally, Dione Clementi and Sandra Ruban show the barons in the late twelfth and thirteenth centuries as shapers of the English Constitution and as real-estate dabblers.

Together, these thirteen essays compose a many-faceted prism. Each creates a reflection through which to view from a different perspective the interplay of tradition and change in the Anglo-Norman world of the eleventh, twelfth, and thirteenth centuries.

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LESLIE ALCOCK. *Economy, Society, and Warfare among the Britons and Saxons*. Cardiff: University of Wales Press. 1987. Pp. x, 343. \$70.00.

This book has two main purposes. First, it presents data collected in the course of archaeological excavations at two important settlement sites of early medieval Britain: Dinas Powys in southern Wales and Cadbury in Somerset, England. Leslie Alcock discusses his own interpretation of the data and the interpretations of others in light of preliminary reports published in the 1960s and 1970s and the considerable scholarly debate that followed. The book provides an up-to-date review of what we know about these sites. Second, the book presents a synthesis and interpretation of available information about social, economic, political, and military configurations among the peoples who inhabited Britain between the end of Roman domination and A.D. 800.

The author relies not only on archaeological evidence to accomplish this task but also on historical data and skillfully integrates both. In several chapters he deals at length with the written sources and devotes particular attention to methodological questions involving use of both archaeological and written documents to address specific questions. Alcock is well qualified for this task, because he was trained as a historian at Oxford University and has been directing archaeological excavations at early medieval settlements in Britain since the 1950s.

The book is divided into five parts. Part 1 offers interpretations of the site of Dinas Powys, including discussion of the excavation results, review of the scholarly reaction to the initial publication of the findings in 1963, and Alcock's new thinking in light of recent archaeological and historical research in Britain and throughout northwestern Europe. Part 2 presents the finds from Dinas Powys, including descriptions and drawings. Part 3 reviews and reappraises Alcock's excavations at Cadbury, a site often associated with the Arthurian Camelot. Alcock addresses the complex question of Arthur's historicity, particularly in relation to the archaeological finds. This part also treats other fortified settlements of the period A.D. 400–800 in Wales and southwest Britain, of which nineteen are listed.

Part 4 discusses interactions, particularly warfare, between Britons and Anglo-Saxons. In part 5 the author provides a synthesis of the early medieval period in western Britain entitled "Economy, Society, and Warfare."

Thirteen of the nineteen chapters have appeared before, as has part of another; five are new. Most of those that have appeared elsewhere were in publications not readily accessible to

American readers; thus, their assembly will be much appreciated by medieval historians. In the new chapters, the author is able to integrate the earlier essays and to explain how and why his thinking has changed.

The great value of this book for historians working on medieval Europe is its demonstration of the rich possibilities of integrating information from texts with that recovered through archaeological research. As Alcock makes clear, social and economic patterns in early medieval Britain were not unique. Recent archaeological investigations elsewhere in Europe, for example at the Runder Berg in southwest Germany and at Helgö in Sweden, show that similar changes in commerce, crafts, and social structure were taking place throughout much of Europe in the centuries following the Roman presence. These developments were critical for the growth of towns after A.D. 800 and thus are important for our understanding of the formation of medieval Europe.

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NIGEL SAUL. *Scenes from Provincial Life: Knightly Families in Sussex, 1280–1400*. New York: Clarendon Press of Oxford University Press. 1986. Pp. viii, 204. \$43.00.

Nigel Saul's book deals with the Sussex knightly families of Etchingham, Waleys, and Sackville. We first meet the families in 1366 at a land transaction. But, apart from the fact that they were near neighbors and of similar social standing, there is little reason for the selection of these three, and they provide a very small sample. The families "selected themselves," Saul says, because of the survival of their archives: otherwise, why not the Dallingridges, the Poynings, or a host of others?

The book is not one of surprises. As we would expect, the families by the thirteenth century were providing sheriffs, keepers of the peace, knights of the shire, and poll-tax collectors. They acted as one another's feoffees except where family self-interest might prove dangerous. (When Sir Andrew Sackville wanted to make sure of the succession for his bastard son, he very carefully called not on his fellow landowners but on his employees and officials who had a vested interest in the continuance of the Sackville line.) They hunted together, and they encountered the same problems of estate management.

But how far are they representative of their class or of their county? In a perceptive chapter on the local community, Saul contrasts Sussex with Warwickshire and Norfolk. Unlike those two counties,

Sussex was not organized in or around a magnate affinity. Furthermore, Sussex had an ancient historical identity (if compared with Leicestershire or Nottinghamshire), but it had no corresponding geographical unity. Chichester lay far to the west, not highly convenient for meetings of the county court. Not until Henry VII's reign were meetings more conveniently held alternately at Chichester and at Lewes. From earliest times the county had been divided into two halves: West Sussex and East Sussex. To some extent the earls of Arundel provided a focal point, but otherwise there were few ties between the knights of the west of the county and the knights of the east. The lands of the three families were found solely in the eastern part of the county, in the rapes of Hastings and Pevensey, although the families did hold land in other counties, including Kent. The Waleys were tenants of the archbishop of Canterbury, whose estates straddled Kent and Sussex. Ties with western Kent may have been stronger than with western Sussex.

Obviously, the records do not allow an equal treatment. There is a fine chapter on Etchingham church, bringing out the religious and social aspirations of the family. Descended from Reinbert, steward of the count of Eu, the Etchinghams were naturally at first donors to the monks of Battle. But by the late thirteenth century they were directing their favors toward the Cistercian house of Robertsbridge, close by their manor of Salehurst. There they were buried (like the Sackvilles at Bayham) until Sir William V built the church at Etchingham in the 1360s. Here, surrounded by heraldic symbols of his status in the stained glass windows, he was to rest near the altar. The impressions of the expectations and life and "death" style of the other two families are less vivid.

Saul is at heart a miniaturist—an exquisite miniaturist. He has chosen a small frame in which to work.

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BARBARA E. CRAWFORD. *Scandinavian Scotland*. (Scotland in the Early Middle Ages, number 2.) Atlantic Highlands, N.J.: Humanities or Leicester University Press, Leicester, England. 1987. Pp. xii, 274. \$60.00.

Scotland in the early Middle Ages was a melting pot for a number of distinct cultural groups. Some were indigenous (Britons and Picts), others invasive (Scots, Vikings, and Angles). Disentangling their contributions to the history of the period poses a considerable challenge for two reasons.

First, the general insufficiency of data has meant that a meaningful survey requires the integrative use of historical, toponymic, and archaeological data. Second, the range of cultures involved, each with its own institutional, linguistic, and material forms, requires that, in order to achieve a national perspective, the interdisciplinary challenge has to be confronted five times over. The scale of these problems has meant that, although many scholars have coped well with the need for an interdisciplinary approach, a substantial amount of critical debate has worked within ethnographic limits, seeking to establish the particular contribution of particular cultures. Barbara E. Crawford's work illustrates this culturally specific approach, being the first modern attempt to fuse the now-extensive research on the Scandinavian ingress into a book-length study.

The historiographical problems surface in the way the work is organized. An initial survey of where the Vikings raided and settled is followed by a lengthy discussion of chronological problems, particularly the questions of when they first appeared in Scotland and when they made the crucial shift from raiding to settlement. The second half of the book pieces together their character as a society and is ordered around the different sources of data. The rich potential of archaeological evidence and place names is noted, but, although Crawford cautions the reader on the problems of using place names and suggests their interpretation is best left to philologists, she does not emphasize the difficulties—the confidence-sapping reversals of interpretation—that even the philologists have highlighted. The sagas are judged critically as sources of hard fact, requiring much panning or sifting in order to extract what she sees as a residue of value.

The strength of Crawford's book lies in its quality and breadth of synthesis. For the first time, scholars are provided with a well-rounded account of the Scandinavians in Scotland. The treatment of five particular themes stands out. First, Crawford provides by far the most authoritative survey of the many questions that surround the early history of the Vikings, from their first appearance as raiders in the late eighth century. Her discussion is particularly strong on why the Vikings were attracted by the northern and western isles and on the use they made of them as bases for raiding and as staging posts in the trade with Dublin. Yet, in stressing the quest for items of ornament and display (for example, precious stones, brooches), Crawford might have made more of the role played by such goods in sustaining chiefly systems of ranking and redistributive exchange. Second, the position of the Orkney earls—in relation both to the Norwegian kings and to those controlling

Viking areas pivoted around the Isle of Man—is defined with considerable skill. I particularly applaud the bold, resourceful way in which Crawford draws out the distinction between the sort of society that developed around the earls in Orkney and Caithness and that found in the Hebrides. Third, she provides a critical review of the many problems of continuity raised by the Scandinavians in relation to both the Picts and the Scots. Again, important differences are drawn between, on the one hand, Orkney and adjacent mainland areas and, on the other, the Hebrides, though I found the suggestion that both Scots and Picts may have survived locally in purely pastoral areas surprising in view of how the Vikings would have used such resources in transhumance. Fourth, there is much to admire in the way Crawford builds up a sense of settlement evolution, the main thrust of her argument being that place names are more reliably used to establish the relative phasing of local settlement growth rather than the absolute chronology of regional patterns of colonization. Fifth, she has much to say on how pagan beliefs yielded before the spread of Christianity. Like other studies of this problem, what she says about the topic has more to do with how pagans died rather than with how they lived. I accept that this reflects the bias in the evidence available, but surely Andre Gurevitch's work could have been used to hint at the rich ideological system that might be obscured by such deficiencies of data. Yet one can hardly complain about matters of omission in a book that succeeds in welding so much disparate material together into such a coherent and satisfying work.

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SVERRE BAGGE. *The Political Thought of the King's Mirror*. (Mediaeval Scandinavia Supplements, number 3.) Odense, Denmark: Odense University Press. 1987. Pp. 253. 250 KR.

To most historians of medieval Europe, the history of the peoples on the geographic margins of Europe often seems as exotic as the history of medieval Europe does to nonspecialists, including nonspecialist historians. Language differences, disparities in chronology and communication, and a fascination with the exotic often shape the views of professionals who otherwise routinely address the medieval worlds of England, France, Italy, the Rhineland, and, to a slightly lesser extent, Iberia and Germany. In the case of medieval Scandinavia, this lack of familiarity is heightened by the lack of recent studies in accessible languages and by the extraordinary riches of the saga literature, which

has long been available in translation. But a combination of sagas and general ignorance does not make for a useful or desirable kind of historical knowledge. Fortunately, in the work of Peter Sawyer, Peter Foote, William Ian Miller, and Ruth Mazo Karras, much medieval Scandinavian history is becoming available to non-Scandinavian historians and in terms all historians are well equipped to understand.

Sverre Bagge's book is a substantial contribution to our knowledge of medieval Scandinavian political thought, made even more valuable by the author's ability and determination to link the thought of this mid-thirteenth-century work with similar expressions of political theory elsewhere in Europe. Bagge cites the work of Wilhelm Berges, Gaines Post, Brian Tierney, and others to accomplish this task.

After dating the work and outlining its circumstances in mid-thirteenth-century Norwegian history, Bagge discusses its thought in a series of effective thematic chapters that facilitate a comparison of the work with others of the period. The foundation of royal power, the king as judge, the ideal king, the king and the church, the king and the people, and the concept of the state each occupy a chapter, and each chapter cites a rich fund of primary sources and secondary works regarding examples from elsewhere in Europe. Bagge's method of exposition thus makes *The King's Mirror* accessible not only in the context of other Fürstenspiegel literature but also in the topical and thematic concerns of medieval political thought in general. She also illuminates those points, circumstantial and historical as well as intellectual, that root the work in Norwegian history as well as in general medieval political theory.

Bagge's work appears as volume 3 of *Mediaeval Scandinavia Supplements*, monographs produced by the best scholarly journal dealing with the subject. It is to be hoped that more will follow of the quality and appeal of Bagge's work.

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S. P. WOLFS. *Middelleeuwse dominicanessenkloosters in Nederland* [Medieval Dominican cloisters in the Netherlands]. Maastricht: Van Gorcum. 1988. Pp. xi, 125. f. 40.

This is a companion piece to S. P. Wolfs's book of 1984 dealing with Dominican friaries in the Netherlands. He includes both the Second Order of Dominicans, sisters who made a profession of the rule of St. Augustine, and the Tertiaries, a term used after 1484 for uncloistered lay people who sought guidance from the Dominicans and had

been known earlier as the "Sisters of the Penitence of St. Dominic." That St. Catherine had belonged to such a group at Siena furthered their diffusion, and by the sixteenth century the Tertiaries often took vows and entered cloisters, becoming functionally similar to the Second Order.

The Dominican nuns never flourished in the Netherlands. Wolfs identifies only twelve foundations. Most were established by pious lay people in the late fourteenth and fifteenth centuries, although the nunnery at Burgh developed from a Beguine community present by 1271, and Westroyen near Tiel evolved from a failed Dominican convent. Information is extremely sparse for 's Hertogenbosch and Reide. The nunnery at Lukswolde may have been projected but was never built, while that at Rotterdam may not have been Dominican. Most information on the other eight—at Asperen, Burgh, Leeuwarden, Leiden, Roermond, Westroyen, Woudrichem, and Wijk bij Duurstede—comes from the sixteenth century. Appendixes provide information on three nunneries that were projected but never built and another five that have been claimed erroneously as Dominican. All were small and inconspicuous and suffered badly during the iconoclastic disorders in 1566. There was little recruitment thereafter. The nuns gradually died or dispersed, generally to other Dominican foundations. Only the nunnery at Roermond survived beyond 1600.

The book is intended as a reference work rather than a history. Separate indexes are provided for Dominicans, Dominican nuns, other persons, Dominican convents, and Dominican nunneries. For each nunnery, Wolfs poses nineteen questions, including who was the patron saint, whether the house was affiliated with the Second or Third Order, what the circumstances of foundation and affiliation with the Dominicans were, as well as inquiries about the buildings, cemeteries, and churches and their eventual fate, peculiarities of religious observance, relations with secular and ecclesiastical authorities, circumstances of dissolution, libraries, seals, a selection of charters and the eventual fate of the archives, and any drawings surviving. Except for what one can gather from the circumstances of their endowments and descriptions of the cloister outbuildings—several had breweries—there is little on how the nuns supported themselves. There are lists of prioresses and Dominican confessors, and full bibliographical and archival references are provided.

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(Veröffentlichungen zur Bayerischen Geschichte und Kultur, number 14/87.) Munich: Bayerische Staatskanzlei. 1987. Pp. 295.

RAINER A. MÜLLER, editor. *Reichsstädte in Franken: Aufsätze*. Volume 1, *Verfassung und Verwaltung*; volume 2, *Wirtschaft, Gesellschaft und Kultur*. (Veröffentlichungen zur Bayerischen Geschichte und Kultur, number 15/87.) Munich: Bayerische Staatskanzlei. 1987. Pp. 386, 391–443.

This coordinated set of three volumes includes two volumes of essays (one on constitution and government, the other on economics, society, and culture) along with a catalogue to the Bavarian state historical exhibit on the smaller Frankish imperial towns that was the occasion for the entire undertaking. The subject of this sizable mass of material, over eleven hundred pages, is the five smaller imperial towns in Franconia: Dinkelsbühl, Schweinfurt, Rothenburg ob der Tauber, Weisenburg im Nordgau, and (Bad) Windsheim. These five towns are all in Bavarian "Franconia" today, but in the old Reich the imperial town of Dinkelsbühl was a member of the Swabian Circle while the others were in the Franconian Circle. In order to focus on these lower-magnitude stars, it is necessary that the dominant sun of Franconian urban life, Nuremberg, be artificially obscured. The stress is thus on certain small and medium-sized towns in a certain region of the Holy Roman Empire between the thirteenth and the early nineteenth centuries, and, although many of the contributors are local historians, they are all interested in using local studies to grasp the concrete realities that often escape us when we choose to pursue "big history." Yet, as if to apologize for the contributors' boldness in examining their subject deeply rather than broadly, the first volume has more than one hundred pages of "general" essays by such authorities as Volker Press ("Die Reichsstädte im Reich der frühen Neuzeit") and the general editor, Rainer Müller ("Quaternionlehre und Reichsstädte"), whose contribution is on a puzzling tradition of describing the estates of the empire in series of fours. Other articles touch on such subjects as the relationship between the Frankish imperial towns and the two supreme imperial courts (the Imperial Aulic Court and the Imperial Chamber Court) and the principles of imperial and circle taxation.

Some of the articles succeed brilliantly at the proper mission of this collection, which is rendering the institutions of premodern communes with startling precision and insight, particularly Reinhard Heydenreuter's "Reichsstädtisches Recht," Uwe Puschner's "Reichshandwerkordnung und

RAINER A. MÜLLER and BRIGITTE BUBERL, editors. *Reichsstädte in Franken: Katalog zur Ausstellung*.

Reichsstädten," and Martin Dallmeier's "Reichsstadt und Reichspost." The single most significant article for nondevotees of Franconian history is a small masterpiece of the precise study of the socialization of space, Peter Höhe's "'Stull, benk und sidel must man han': Wohnen in der Reichsstadt Weissenburg vor dem Dreissigjährigen Krieg," which studies the distribution of furniture, utensils, and tools among the rooms in the inventories of small households.

There is a studied symbiosis between the essays and the catalogue of the exhibit, since the subsections of the exhibit were often organized by the very authors of the later essays. The result is that the catalogue can be used in some cases as a key to the essays. This is most successful in the treatment of hospitals, where these imposing endowed establishments can be studied in artifacts and plans in the catalogue ("Spital und Krankenversorgung") and then in essays, particularly Ulrich Kniefelkamp's "Die Heilig-Geist-Spitäler in den Reichsstädten" and Christian Probst's "Das Medizinalwesen der Reichsstädte." The illustrations for the catalogue section on the Jews ("Gemeinde der Juden") are particularly apt and striking, although the companion article is not one of the stronger in the collection: Ludwig Schnurrer's "Die Juden in den kleineren fränkischen Reichsstädten." The illustration on page 103 of the catalogue, a well-known advertising placard painted by Ambrosius and Hans Holbein in Basel, seems to have nothing to do with Franconia. In all, though, the catalogue is a valuable introduction to the visual vocabulary of German urban life.

Some articles can entice an unwary student to take a path into the unknown, such as Helmut Zedelmaier's "Stadtbeschreibung als literarische Tradition: Die fränkischen Reichsstädte in der kosmographisch-geographischen Literatur der frühen Neuzeit," which seeks to describe the genre of the town blurb in travel and geography books.

The individual contributions add up to a considerable body of exciting work, but to what end? The first goal of the project is to win a clearer grasp of small-town life in premodern Germany, which Mack Walker long ago undertook in English in his *German Home Towns* (1971) (his paradigm was, in fact, Weissenburg im Nordgau). But, while Walker spoke of urban life as a whole, this project concentrates specifically on imperial urban estates (with a brief foray into the history of the curious Franconian "imperial village" of Gochsheim). This implies the old legalistic assumption that only an imperial town developed "naturally," a concept long ago rejected by Walker. A typology of urban life in general has to be the next step for urban research in Germany, although it will upset deeply held beliefs.

Such a lavish undertaking should never be brought to press without an index. The lack of such a tool, even for the two volumes of essays, means that what was conceived as a collective undertaking will never be able to have a collective impact.

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MAJA PHILIPPI. *Die Bürger von Kronstadt im 14. und 15. Jahrhundert: Untersuchungen zur Geschichte und Sozialstruktur einer siebenbürgischen Stadt im Mittelalter.* (Studia Transylvanica, number 13.) Cologne: Böhlau. 1986. Pp. 335.

Maja Philippi has written a major work on the history of Kronstadt (Braşov). The scope of the study transcends that of the subtitle in that the social structure of the Transylvanian town is placed in the broader context of cultural and political conditions and values prevalent in Transylvania and in other Germanic towns of the fourteenth and fifteenth centuries.

The author devotes most of the work to a detailed and sophisticated analysis of the social structure and civilization of Kronstadt, paying equal attention to the burghers, the upper clergy, the artisans, the intellectuals, and the urban poor as well as to the interaction of the various groups. Much of the data presented and analyzed is derived from previously unexplored sources contained in the state archives of Braşov and Sibiu (Hermannstadt) and, equally significantly, of the Black Church of Braşov. Philippi demonstrates that medieval Kronstadt, despite the fact that the German colonization of Transylvania had been primarily by peasants, was from its inception a commercial town whose burghers were concerned almost entirely with economic activities compatible with the feudal system. Philippi dismisses the oft-held view that Kronstadt, like other Transylvanian towns, was established primarily for military purposes by proving that the town's struggle for political autonomy, self-jurisdiction, and other such privileges was a function of the burghers' primary goal of economic emancipation. She draws parallels between German and Magyar towns, emphasizing, however, the differences rather than the similarities. Abundant data are also provided on the relations between Kronstadt and the Romanian provinces of Moldavia and Wallachia that reinforce contemporary views held by Romanian historians.

This book is devoid of standard polemics regarding historic rights in Transylvania, presumed Ottoman intolerance of economic activities, rival

linguistic claims, and all other dubious contentions that have traditionally distorted the reality and understanding of the history of Transylvania in general and of Kronstadt in particular.

The study, remarkable by its scholarship, intelligence, objectivity, and originality, is indispensable to all students of medieval urban history and of Transylvanian civilization prior to the Habsburg-Ottoman confrontations of the sixteenth century.

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MARTIN AURELL. *Une famille de la noblesse provençale au Moyen Âge: Les Porcelet*. Foreword by NOËL COULET. (Archives du Sud.) Aubanel: Archives du Sud. 1986. Pp. 217.

This study, an abridged version of the author's *thèse de troisième cycle*, traces one of the premier aristocratic lineages of Provence from its obscure origins in the late tenth century until its "decadence" in the fourteenth. Although the Porcelets themselves are always the focus of interest, their rise and fall become the context within which to understand the fortunes of the Provençal aristocracy in the High Middle Ages.

The ancestor of the family, Daidonat, first emerged in the 970s as one of the notables of Arles in the entourage of Count William II of Provence. Until the fourteenth century, his descendants remained tied to the urban world of Provence, residing in the Vieux-Bourg section of Arles, which they controlled, and which provided much of their urban wealth. Their rise into the aristocracy resulted from their continuing support of comital authority. Into the first third of the thirteenth century, they were faithful and well-rewarded supporters of the Barcelona counts and profited from their position to expand their estates in the Camargue, the Crau, the étang de Berre, and the Alpines. A matrimonial policy supported this rise. Porcelet daughters married up into old Provençal aristocracy, and Porcelet men married down to daughters of consular notables of Arles, whose dowries further enriched the family. After an initial alliance with the count of Toulouse in the early thirteenth century, they became equally loyal supporters of Charles of Anjou. However, declining economic resources and impotence before the Angevin political system reduced them by the early fourteenth century to the status of dutiful functionaries.

Martin Aurell attributes this decline to the steady erosion of the family's financial fortunes, which he links to a change in inheritance strategy. While in the eleventh and twelfth centuries one

heir was favored in each generation, from 1220 family property was more equally divided among all male children, thus leading to the dissolution of the family's patrimony.

The author's careful analysis of marriage patterns, property holdings, and political maneuvers confirms in general the outline of meridional society traced by Pierre Bonnassie, Cinzio Violante, Jean-Pierre Poly, and others. Where Aurell differs, it is generally to present a more conservative interpretation of the relationship between the Porcelets and the church. He disputes Poly's contention that the Porcelet acquired the Vieux Bourg through simony, arguing instead that it was a comital grant. He categorically rejects any class or economic analysis of the antiepiscopal revolt of 1236 (which he curiously calls anticlerical). In both cases his evidence is at best ambiguous.

The strength of the volume is Aurell's painstaking collection and analysis of 606 charters in which the Porcelets appear between 972 and 1320. Using this corpus, he attempts to discuss not only the political fortunes of the Porcelets but systems of kinship, patronymics, involvement with the church, and other more intimate aspects of the family's history. If, in spite of this attempt, the reader is left wondering about the self-perceptions, strategies, and values that animated this Provençal family, the fault lies not with Aurell but with the laconic nature of his sources.

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TREVOR DEAN. *Land and Power in Late Medieval Ferrara: The Rule of the Este, 1350-1450*. (Cambridge Studies in Medieval Life and Thought, fourth series, number 70.) New York: Cambridge University Press. 1988. Pp. xiv, 212. \$44.50.

This book begins with a seeming paradox. Just as northern European historians have begun questioning the significance of feudalism in medieval political and social life, Italian researchers are belatedly recognizing its importance to seignorial rulers. As Trevor Dean puts it, "Feudal lordship is thus seen to have a reality in Italy which runs counter to the trend in northern Europe to minimize its significance or to write as if fiefs and vassals had never existed" (p. 5). He then sets out to verify this statement in his study of the Este of Ferrara.

In an initial sketch of Ferrarese history to 1450, Dean describes the success and longevity of the Este principality. He provides an excellent survey of the lands of the Este, a difficult task given the fragmentary nature of the evidence. He is especially successful in showing how the family

usurped control of ecclesiastical lands. Though gradually withdrawing from their original base of power in the Padovano, the Este expanded south and west into the territory of Modena and Reggio.

Dean then convincingly demonstrates that the Este used their holdings to create a network of supporters. By the early fifteenth century, the Este court included vassals from throughout north-central Italy, as the Este granted fiefs to non-Ferrarese vassals. On the other hand, they were less generous with old Ferrarese noble families. Certain patterns are discernible in the granting of fiefs. Administrative officials in particular were rewarded with fiefs, and strategic locations were assigned to loyal courtiers. In addition, Dean identifies many small fiefs granted to artisans, merchants, and domestic servants. Enfeoffment and vassalage permeated the Este administration and kept the focus of political life firmly on the court. Este grants had an overwhelming influence in determining status for the noble class in Ferrara.

It is apparent that the major source of social advancement was service to the Este. Knighthood was a coveted symbol of the highest status. Conversely, uncooperative rural nobles were often forcibly dispossessed of lands that were then granted to Este supporters. Dean argues that the feudal contract was not an empty or anachronistic document but one that had great importance for the Este and their vassals. His documentation of the dramatic rise and fall of the Roberti family of Reggio exemplifies the fate of noble families who forfeited the trust of the Este. When enfeoffment of rural nobles was not possible, the Este resorted to the *accomandigia* contract to assure military aid. His discussion of this contract is enlightening.

This is the most thorough study to date of feudal politics in Ferrara. Dean's use of the archival sources is extensive, and he provides frequent excerpts from the documents to make his points. He also puts Ferrarese feudalism in a comparative context, pointing out parallels and differences with practices in England, France, and other Italian cities. One only regrets that Dean did not extend his study past 1450. In a recent article, Richard Tristano argues that Borso d'Este (1450–71) carried out major changes in the Ferrarese nobility by changing the pattern of feudal grants (*Medievalia et Humanistica* [1987]: 43–64). One hopes that Dean will continue his study in another book in order to analyze this interesting transition.

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JOHN DAY. *La Sardegna sotto la dominazione Pisano-Genovese: Dal secolo XI al secolo XIV*. (Storia degli

stati italiani dal medioevo all'unità.) Turin: Utet Libreria. 1987. Pp. 199.

John Day surveys the history of Sardinia from the rise of its four indigenous kingdoms, through the twelfth and thirteenth centuries as Pisans and Genoese struggled to dominate the island, down to the conquest by the kingdom of Aragon in the fourteenth century. Sardinia itself, aptly described as a poor, turbulent, and unhealthy place, became the object of colonial warfare as external powers sought to acquire the few riches—salt, silver, and grain—that the islanders could provide. This history, from the point of view of Sardinians, is an unhappy one, and Day recounts it with great verve and sympathy.

The four themes of this book are demography, the four kingdoms, society, and colonial strategies. There are abundant sources for the study of population, especially for the later period, and the findings bring to life the horrible plight of the inhabitants. In the best of times, Sardinia suffered from terrible poverty. Low birth and high death rates, poor internal transport, deplorable hygiene, and endemic malaria all conspired, according to Day, to create a passive, indolent, and fatalistic people. Day shows in a sophisticated way how patterns of internal migration, over-reliance on rearing livestock, unjust tax policies, shortage of cash, and heavy compulsory labor services compounded these grim realities. The high mortality caused by the bubonic plague simply concentrated what little wealth there was into fewer hands and left the bulk of the survivors no better off; the poor have nothing to leave their heirs.

It is hard to believe that anyone would think that this Sardinia was worth having, but the Pisans and Genoese saw possibilities and divided the island into spheres of influence in 1169. The next centuries witnessed the mainlanders' efforts to undermine local authority and increase their own. Desperate conditions on the island actually made it possible to export its low-priced grain. Cheap labor fostered the development of extensive salt works and silver mining. Day does not sufficiently emphasize the traffic in people; it is likely that the first merchants to be interested in Sardinia came as slavers—such was the fate of the island. The emerging colonial economy enriched foreigners settled in a few ports and contributed to keeping most inhabitants at the margins of subsistence.

For reasons having nothing to do with Sardinia, Pope Boniface VIII gave the island to James II of Aragon in 1297. The Catalans did not arrive in force until 1324. Day delineates the appalling treatment the island then received. Spanish rule proved durable and severe, and it succeeded in prompting the first stirrings of Sardinian nation-

alism. (Some comparisons to the Catalan policies in Majorca would have been helpful.)

The older scholarship emphasized the civilizing and positive features of colonial rule; Day proves that it did Sardinia little good. The Catalans, however, learned from their failures some lessons about how to create a colonial economy. John Day has written an excellent book that will be the basic point of departure for studying Sardinia and colonial experiments.

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JONATHAN RILEY-SMITH. *The Crusades: A Short History*. New Haven: Yale University Press. 1987. Pp. xxx, 302. \$25.00.

This ambitious attempt to put all of the Crusades into the context of the times and to explain their relationship to one another opens at Clermont in 1095 and closes after the Turkish siege of Vienna in 1683. Although it is impossible for any one scholar to be fully acquainted with every episode, Jonathan Riley-Smith makes a commendable effort. More important, he gives us new insights.

The essential crusading experience was not the isolated military expedition but the "perpetual crusade." In regions where Christendom confronted an Islamic or pagan enemy for decades or even centuries on end, it was impossible to obtain a papal bull for every emergency. In those "frontier societies" the Roman Christians adapted their traditions to meet practical problems. This approach disturbed newcomers arriving from the homeland and still frustrates those modern historians who expect to find in the crusading states in Spain, the Holy Land, and Eastern Europe familiar Western feudal practices, religious attitudes, and military traditions.

Riley-Smith is weakest in describing the Eastern Crusades. The battle of Durbe involved both the Prussian and Livonian branches of the Teutonic Order. An analysis of the participation of the Prussians after 1283 cannot simply divide them into "serfs" and "a few collaborators." The Battle on the Ice in 1242 was much more complex than Riley-Smith describes and is worth more than a passing reference because many American scholars perpetuate the propaganda of the classic 1938 film *Alexander Nevsky* by associating the traditions and ambitions of the Teutonic knights with those of the Nazis. The Mongol invasion is presented in these words (p. 164): "In 1241 Poland and Hungary were invaded and a German army was defeated at Legnica [*Liegnitz*], although the death of Genghis Khan's successor, Ögöдай, in 1242 interrupted the invasion of central Europe and may

indeed have saved it." Despite occasional inaccuracies and confusing wordiness, Riley-Smith at least seeks to cover important crusading activities that other short histories ignore.

The digressions on the fortunes of the crusading states and the varieties of crusades are most welcome essays, arguably the best written and most original sections of the book. Riley-Smith goes on to say that the political diversions and misuse of crusading energies may have impeded the defense of the Holy Land but did not kill the crusading spirit. Popular interest in the Crusades dimmed very slowly as people came to think the "just war" more appropriate for Christians than the "holy war."

A generation ago no historian could have written a book of this breadth and quality. It was not then customary to think of the Crusades as extending much beyond the nine principal expeditions to the Holy Land, and there was much about those campaigns that was poorly understood. Specialists will know how much Riley-Smith himself contributed to the changed situation of today.

The bibliography is augmented with short comments to guide further reading. The citations are sparse (initials of authors, no page numbers for articles) and may occasionally earn users of interlibrary loan a snippy request for "more information."

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MODERN EUROPE

ULRICH-CHRISTIAN PALLACH. *Materielle Kultur und Mentalitäten im 18. Jahrhundert: Wirtschaftliche Entwicklung und politisch-sozialer Funktionswandel des Luxus in Frankreich und im Alten Reich am Ende des Ancien Régime*. (Ancien Régime, Aufklärung und Revolution, number 14.) Munich: R. Oldenbourg. 1987. Pp. viii, 306.

Luxury was a cornerstone of the Old Regime. If middle-class *ressentiment* once inspired historians to portray luxury in the light of princely profligacy and aristocratic dissipation, more dispassionate commentators have stressed the extent to which conspicuous consumption in the Old Regime was central to the visual articulation of social rank and political preeminence.

Ulrich-Christian Pallach's study examines the social, economic, and political significance of the luxury market in eighteenth-century France and Germany. Pallach has attempted to write an *histoire des mentalités*, one that documents what he sees as a profound shift in attitudes toward luxury begin-

ning in the 1770s. Economic historians, however, will profit most from his investigation. Focusing primarily on the silk, porcelain, furniture, and tapestry markets, Pallach begins by examining the balance, structure, and geography of the French and German luxury trade. He looks especially at the effects of famine and rising grain prices, and he concludes that certain sectors of the luxury market may well have benefited from the subsistence crises that recurred after 1770. Noble proprietors who profited from higher grain prices, after all, were able to increase their consumption of luxury goods. In France falling demand for luxury goods reflected less the impact of agrarian crisis than of declining exports to Germany (where domestic production of luxury wares had risen), stylistic restraint introduced by neoclassicism, and periodic austerity measures adopted by the French court. Pallach has consulted a wide range of French and German archival sources and pamphlet literature, and his case studies of markets in Paris, Lyon, and Sèvres help illuminate the obstacles and opportunities that confronted eighteenth-century merchants of luxury goods.

Examining the changing social and political functions of luxury, Pallach argues that the mercantilist promotion of the market, spurred by the representational needs of the French court, helped create an autonomous material culture that ultimately served to reduce the symbolic potency of luxury. The austerity measures of the court that followed in the wake of the fiscal and agrarian crisis of the 1770s further hastened this process of *Entzauberung*, depriving the crown of a crucial medium of control and charisma. In Germany, where the "symbolic capital" of court luxury was less concentrated owing to the political fragmentation of the empire, the decline of luxury as a charismatic reservoir was of less consequence. By the 1770s, furthermore, it was relatively spartan Potsdam, not Versailles, that set the tone for German court life. What German practitioners of princely austerity lost in charisma, concludes Pallach, they made up for in bureaucratic rationality and efficiency.

Here Pallach's Weberian contrast between charismatic and bureaucratic modes of legitimation is far too blunt an instrument for dissecting the differences between French and German court cultures. Drawing heavily on the standard works of Norbert Elias and Jürgen Freiherr von Kruedener, Pallach overlooks more recent studies by C. B. A. Behrens, Reinhard Bendix, Pierre Bourdieu, and Albert Hirschman that might have enriched his analysis. The author's repetitious style, his sometimes acrobatic leaps between markets and mentalities, and the absence of an index also cripple the effectiveness of this book. Pallach, in

effect, has written two books on eighteenth-century luxury, one economic in focus and the other cultural. I suspect that historians of luxury will find the economic analysis more useful.

JAMES VAN HORN MELTON
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JEAN-CLAUDE HOCQUET. *Le roi, le marchand et le sel*. (Actes de la Table Ronde, L'Impôt du Sel en Europe XIII^e-XVIII^e siècle, 1986.) Lille: Presses de l'Université de Lille. 1987. Pp. 376.

Jean-Claude Hocquet is our foremost historian of salt—its production, distribution, taxation, and consumption. The author of a monograph on the role of salt in Venetian history (*Le sel et la fortune de Venise*, 2d ed., 1982) and of a general history (*Le sel et le pouvoir, de l'an mil à la Révolution Française*, 1985), he presents here the papers of a 1986 conference on the evolution of salt taxes from the thirteenth to the eighteenth centuries. Hocquet's introductory and concluding chapters provide some cohesion to the disparate case studies taken from France, Italy, Spain, Austria, Germany, Poland, and the Netherlands.

The title of this collection is slightly misleading insofar as we hear little about kings and merchants, save in Michel Mollat's essay on Jacques Coeur's salt ventures and Marjolein 't Hart's study of the seventeenth-century salt trade in the Low Countries. The papers focus rather on the administration of salt taxes. Although we tend to think of salt chiefly as a food preservative in preindustrial Europe, it was also required in the preparation of cheese, cloth, and leather, and contemporaries regarded it as a vital seasoning as well. Salt was universally taxed precisely because it was an indispensable commodity.

The French gabelle, the textbook paradigm of a salt tax, naturally receives a large amount of attention in this volume. The gabelle was, of course, more than a tax; it was the obligation to purchase a prescribed amount of salt for domestic use at a fixed price from an authorized depot. Alain Venturini reminds us that Charles I of Anjou imposed the first gabelle in Provence in the 1250s, a full century before the royal gabelle was instituted, and Henri Dubois shows how the duchy of Burgundy followed the royal example in the late fourteenth century. Although salt was privately owned and produced in France, it was distributed and taxed under a bewildering variety of regimes where exemptions and privileges abounded. In a number of cities identified by Bernard Chevalier, local merchants acquired monopoly rights to supply the royal salt grange.

Since gabelle rates were arbitrary and varied by

region, tax farmers developed intrusive practices to combat evasion, fraud, and contraband. Micheline Huvet-Martinet explores the widespread production of contraband salt in western France, which persisted despite patrols by brigades intent on enforcing the gabelle, and Georges Clause analyzes prosecutions for gabelle fraud in Champagne, where salt from Lorraine cost one-third the legal rate. The inherent inequity of the gabelle, the evils wrought by tax farming, and the erection of internal fiscal frontiers all reflect the well-known administrative chaos of the *ancien régime*. Yet, given the amount of revenue at stake and the complexity of the gabelle apparatus, any reform would have entailed a wholesale fiscal reorganization unthinkable at the time.

The remaining papers in this collection are so varied as to defy a brief summary, but they serve as a useful foil to analyses of the better-known French system. Miguel Angel Ladero Quesada summarizes recent research on salt taxes in Castile, where the ownership and production of salt remained a royal monopoly. Harald Witthöft studies the salt-producing city of Lüneburg, which sold salt throughout the Baltic, and Kuno Ulshöfer surveys the fortunes of Schwäbisch Hall, which supplied the Rhine and Neckar. In both cities, merchant oligarchies controlled all phases of the production, taxation, and exportation of salt. Patrizia Angelucci-Mezzetti relates a different experience in Siena, whose economic foundation lay elsewhere: the commune originally intended only to tax the sale of salt but became a monopolistic producer and importer in order to assure its growing population a dependable supply of an essential commodity.

A general history of salt policies clearly must await further archival research. In the interim, Hocquet has drawn attention to the pervasive, and often neglected, ramifications of these policies for social and institutional history.

THEODORE EVERGATES
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CHARLES A. JONES. *International Business in the Nineteenth Century: The Rise and Fall of a Cosmopolitan Bourgeoisie*. New York: New York University Press. 1987. Pp. xi, 260. \$45.00.

As the dual title suggests, this is really two books in one. The more successful of the two traces the rise and transformation of various merchant houses operating within the British trading empire during the nineteenth century. According to the author, Charles A. Jones, an expert on British enterprise in Latin America, these firms emerged and prospered between the 1780s and the 1840s as a

result of the breakdown of the old mercantilist trade system and the advent of British industrial power. In the second half of the century, however, technological and legal innovations, such as regular steamer service, transoceanic telegraphic communication, and liberalized incorporation, brought forth new competitors to challenge the established position of these houses. In examining the merchants' response, Jones distinguishes between those on the "periphery" of the world trade system (in places such as Buenos Aires, Bombay, Hong Kong, and Sydney) and those at the "center" (meaning England and, especially, London). Peripheral firms, he argues, attempted to defend themselves by specializing in commodity trade or commercial banking or by investing in physical infrastructure (port facilities, railroads, power companies). But, in all these businesses, peripheral firms were at a disadvantage relative to "center" firms, which had better access to technology and capital. Although some peripherals survived as independent players, most were eventually drawn into London-based syndicates, such as the London and River Plate Bank or Marcus Samuels's "tank syndicate" (precursor of Shell Oil), almost all of which came to be dominated by center interests. By the early twentieth century, the peripheral firms were suffering, in Jones's words, "progressive marginalisation" (p. 179). Still, the diverse activities of these far-flung merchant houses over the previous hundred years had helped to lay the foundations for modern multinational enterprise—a development, Jones points out, that heretofore has been attributed almost exclusively to large-scale manufacturers seeking wider markets.

Jones's treatment of international business in the nineteenth century is generally convincing because he assembles an enormous amount of illustrative detail to support it (perhaps too much). By contrast, his second "book," identified in the subtitle, although provocative, is less convincing because the supporting evidence is much less conclusive. Specifically, Jones argues that, with the rise of the new merchant houses of the early nineteenth century, there emerged a new social class, the "cosmopolitan bourgeoisie," bound together by business and kinship ties as well as by the ideology of radical liberalism and free trade represented in the thought and career of Richard Cobden. Jones clearly views this class as a force for good and laments its "fall" in the late nineteenth century when, amid the economic pressures mentioned above, it supposedly fragmented into various national bourgeoisies, abandoned internationalism, and embraced the bellicose imperialism of the (aristocratic) ruling elite. However, Jones's documentation of this ideological shift is sketchy.

Equally sketchy is his discussion of the kinship networks underlying the cosmopolitan bourgeoisie. Ties across regional boundaries—between, say, the merchants of Buenos Aires and the bankers of London—are not well delineated. The author is probably onto something important, but he needs to map out the specific business and family connections much more thoroughly. In its present form, what he offers is less a social history than suggestions, albeit useful ones, for future research.

In sum, this is an uneven but highly imaginative work. The author goes in too many directions at once, throwing out too many ideas that are never developed. A classic case, perhaps, of reach exceeding grasp. Nonetheless, there are important arguments here that scholars will have to take into account in all subsequent work on the social and organizational history of international enterprise. Economic and business historians in particular should give this book a careful reading.

MICHAEL S. SMITH
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JEREMY BLACK. *The Collapse of the Anglo-French Alliance, 1727–1731*. New York: St. Martin's. 1987. Pp. xvi, 224. \$35.00.

The death of George I made for considerable confusion in English and in European affairs as well. To whom would the new king listen? How large a personal role would he choose to play? In which departments would he be most active? George II soon turned his eye to foreign affairs. In 1727, Britain's major alliance was with the French, an alliance in which Britain was the junior partner. Cardinal Fleury might complain that the English ought to pay more, as the richer of the two powers, but on most other issues he seems to have had his way.

A French alliance in those years left George II to face the possibility of hostile action by any one of the three eastern powers—Austria, Prussia, or Russia. This alliance was also deeply unpopular with the general public, whose opinion counted for much, and it did little to help the British in their dealings with the reviving power of Spain. Something must be done, cried the impatient king: something, anything. Negotiations were attempted with all the great and many of the secondary powers of Europe in turn, to little or no effect. At first, the Treaty of Seville, concluded with Spain, looked like something, but the Spanish soon repudiated it. In the spring of 1731, the British concluded a treaty with Austria, on terms very favorable to the Austrians but at the cost of their friendship with France. It may be that the

Second Treaty of Vienna secured Hanover from Prussian attack. But it did so, Jeremy Black maintains, only at the cost of isolating the governments of George II in the later 1730s.

Black has searched widely and well in domestic and an impressive number of foreign archives to find material for his study. If he does not read Dutch, or use Spanish, he more than makes up for it by his discoveries in the archives of the minor German and Italian courts. His use of this material is fascinating in itself, showing how the most insignificant German problem ties in with major French and Spanish ones. But he is also careful to connect diplomatic issues to domestic political ones, so that he presents us with almost two monographs, one on the state of Europe and the other on the government of England. The picture that emerges is a complex one. No single individual at this time could be sure of dominating the formation or execution of policy, and to claim any such power for Horace Walpole, or the duke of Newcastle, or the queen, is reductionist. This book may be hard reading, but it is well worth the effort.

STEPHEN B. BAXTER
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CHARLES B. SCHMITT *et al.*, editors. *The Cambridge History of Renaissance Philosophy*. New York: Cambridge University Press. 1988. Pp. xiii, 968. \$79.50.

This is the third in the series of Cambridge histories of philosophy, following *Later Greek and Early Medieval Philosophy* (1970) and *Later Medieval Philosophy* (1982). W. K. C. Guthrie's three-volume *A History of Greek Philosophy*, also published by Cambridge University Press (1962–69), has served as coverage for the earliest period. The structuring varies in all four volumes. Guthrie's works and the volume on later Greek and early medieval philosophy are organized according to single figures within themes and schools, which I prefer. The histories of the later medieval and Renaissance periods are organized by branches of philosophy. The Cambridge history of the later Greek and early medieval period is thin on Plato and Aristotle, strong on Neoplatonism and patristic thought. Although it reaches Anselm as a medieval Platonist, the gap between it and the volume on later medieval philosophy is notable. The editors of the later medieval history designedly emphasized the aspects of medieval philosophy of most direct interest to contemporary philosophizing; as a consequence there is minimal attention to the great scholastic theologians of the twelfth and thirteenth centuries.

The current volume attempts a broader, less

anachronistic approach. In this it follows the conceptions of the chief editor, Charles B. Schmitt, whose regrettable death in 1986 left final responsibility for bringing out the nearly ready volume to Quentin Skinner and Jill Kraye. The broader approach was also sought to handle the problem of the conflicting claims for humanism or scholasticism to represent the "philosophy" of the period. Theories of rhetoric, poetics, and history have been included, but philosophical aspects of music, literature, and law, at one point intended, were not. Art theory, aesthetics, and economic thought are also excluded, although political philosophy survives. "We see the resulting volume not just as a history of what might nowadays be considered the more 'philosophically interesting' aspects of the subject, but as a more broadly based introduction to the intellectual world of the Renaissance as a whole" (p. 3).

There is, however, an unspoken compromise in the composition of the volume, reflecting the differing visions of contemporary Renaissance scholarship and, to some extent, the differing approaches between those of the contributors who are historians by profession and those who by discipline and training are philosophers, however much their scholarly activities have been historical in nature. For example, part 1, "The Intellectual Context," deals not with the background of late medieval philosophy as it led into the Renaissance but with the utilization of manuscripts, the impact of printing and censorship, and with the problems of translation from Greek and the viability of scholastic terminology, all presented in short, informative, and convincing chapters by three historians: the late John D'Amico, Paul Grendler, and Brian Copenhaver. In addition there are chapters on "The Renaissance Concept of Philosophy" by Cesare Vasoli and on "Humanism" by Paul Oskar Kristeller. Vasoli carefully portrays the emergence of Renaissance philosophy as intertwined, if not identical, with that of Renaissance humanism beginning with Petrarch and unfolding through Quattrocento humanism, Renaissance Platonism, the anti-Aristotelian philosophies of nature, and new methodologies. Kristeller presents, in an incisive survey of the origins and history of humanism, his well-known distinction between the two traditions, stressing the importance of humanist contributions to philosophy through their translations and other activities but holding these to be indirect ones and not philosophy itself. Where some humanists do make direct philosophical contributions, they either do so in their traditional rhetorical role as moral philosophers, or they cease to be humanists in their specific philosophical writing and adopt the role of philosophers.

The editors adhere generally to the Kristeller position. Thus, they place Jill Kraye's comprehensive review of ethical philosophies, which were predominantly humanistic, among the major divisions of philosophy. But, in several of these divisions in part 2, "Philosophy and Its Parts," the core of the volume, they include two chapters, one on traditional (that is, scholastic) and one on humanistic or on new Renaissance approaches. E. J. Ashworth writes on traditional and Lisa Jardine on humanistic logic. William A. Wallace covers traditional natural philosophy and Alfonso Ingegno the new philosophies of nature, not humanistic but usually sympathetic to humanism. Quentin Skinner on political philosophy is paired with Kraye, both of them predominantly humanistic. Under psychology Katharine Park's chapter on the organic soul reports some humanist influence, while Eckhard Kessler's chapter on the intellectual soul is purely scholastic. Charles Lohr's impressive treatment of metaphysics is confined to Renaissance Platonism and discussions of Aristotelianism. But Antonino Poppi's treatment of fate, fortune, providence, and free will (under "problems of knowledge and action") deals both with humanists such as Petrarch, Coluccio Salutati, Lorenzo Valla, and Giovanni Pontano and with the scholastic Pietro Pomponazzi's *Di fato*, finishing with brief mentions of Erasmus, Martin Luther, Philipp Melancthon, and John Calvin, all of them more humanist than scholastic. In the same section Richard Popkin's brief chapter on theories of knowledge includes humanism among the elements leading to skepticism and the rejection of traditional epistemologies. Nicholas Jardine's treatment of the epistemology of the sciences includes scholastics and mathematicians but clearly no humanists. A final section on "philosophy and the humanistic disciplines" has Brian Vickers covering rhetoric and poetics and Donald Kelley expounding the humanistic bases of the new theories of history, both chapters centrally humanistic. In two appendixes Anthony Grafton surveys the availability of ancient philosophical works, and Schmitt discusses the emergence of philosophical textbooks at the turn of the sixteenth century.

This volume, which judiciously mixes the scholastic and the humanistic, is without question strongly focused on the discipline of philosophy, as it purports to be. It is a very admirable and certainly very valuable aid to the profession of history. Its distinguished contributors have without exception given it thoughtfulness and precision. Although there are omissions of some figures, its coverage is thorough and reliable. It is, however, in its construction, a somewhat limited "introduction to the intellectual world of the Ren-

aissance" (p. 3), such as it aspires to be, for it provides little place for either religious or scientific thought. It is as though it was the mentality of these editors, as it was explicitly that of the editors of the previous late medieval volume of this series, that theology and cosmology are not legitimate aspects of philosophy (though they found room for an outstanding chapter on astrology and magic by Brian Copenhaver). Some theology does come in as "science of God" in Charles Lohr's fertile chapter on metaphysics, but it is confined to the Lullian and Platonic traditions. Also under "science of being" Lohr explores theological motivations for the anti-Aristotelianism of the period. But there is not much more of either theology or the sciences.

Another discomfort is that in seeking to avoid "the tendency . . . to equate the most important with the most novel features of Renaissance philosophy" (p. 2), the editors have managed to avoid giving any sense of what was important or admirable. In their care not to be judgmental there is a certain grayness of tone, and in the mass of detail in segregated categories individual thinkers are fragmented. As a consequence one has to search with the index in half a dozen places to put together an adequate picture of, let us say, Valla or of other figures whose work is not easy to divide up into categories imposed either by Aristotle or a modern philosophical curriculum. One also loses any clear sense of how the influence of antiquity operated to change or free up or even to impose new rigidities on the thinking (philosophical and other) of those times.

CHARLES TRINKAUS
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MARGO TODD. *Christian Humanism and the Puritan Social Order*. (Ideas in Context.) New York: Cambridge University Press. 1987. Pp. x, 293. \$39.50.

This is an ambitious book that deliberately invites comparison with some other ambitious books—with R. H. Tawney's *Religion and the Rise of Capitalism*, for example—but not always to its advantage. Margo Todd would like to revise, and even reverse, some traditional views about the social ideas of Puritans and others, chiefly to give priority to the Christian humanists in all matters of reform and to assign responsibility for the Civil War to the Laudian reaction against those ideas. She believes in a common Erasmian consensus among Elizabethans, which, had it survived, might have forestalled the revolution. Todd may be right, but her reach, it seems to me, exceeds her grasp, and she raises more questions than she

answers. Yet this is a serious book that shows considerable learning and that will offer some new information, along with much provocation, to every reader.

Although the book is a contribution to a series entitled "Ideas in Context," it is hard to detect much context for its ideas. One sentence alone (p. 174) takes any notice of the "actual socioeconomic circumstances," like "demographic and urban expansion, commercial changes, rising prices," and so on, that were admittedly crucial to the social thinking of the period. From the time of Todd's hero, Erasmus, to the Civil War, there is little sense of development in social thinking—only the rather mindless regression of the Laudians. Nor is the intellectual context much more obvious. Puritans arrive on the scene somehow and borrow heavily from the humanists, but *Piers Plowman* serves as the chief medieval background, and Calvinism appears pretty much without Calvin, Geneva, or London, without Marian exiles, Presbyterians, or predestination. Todd is careful about her definition of "puritanism," but she deliberately excludes theology from consideration, despite (or because?) it was on that ground that Protestants and humanists profoundly disagreed. But she is less careful about "Christian humanism," which usually means Erasmus but sometimes includes John Colet, Thomas More, Thomas Elyot, and others (not Roger Ascham, however, or John Leland), although these men held different views about the very matters that Todd finds crucial. Thus Thomas Elyot in his popular *Book of the Governor* accepts and endorses the hierarchy of birth that the humanists, we are told, rejected; while anyone who has read the first book of *Utopia* and its last few lines will know that More was hardly optimistic about the practical possibilities of social reform.

Todd tries to show that humanist ideas were transmitted to Puritans by concentrating on the universities. Here she provides some new information and a welcome bibliography of manuscript sources that many will find the most useful thing in the book. She might have helped her cause more, however, by emphasizing the transmission of humanist ideas in the grammar schools and outside the universities, since those medieval institutions were particularly recalcitrant to the new ideas. She does no better than anyone else in explaining the "revival" of scholasticism there, which she dismisses as "abortive," although humanists and others kept complaining about it for a hundred years. Nor does it seem obvious that Erasmus would have welcomed the educational reforms of the Puritan revolutionaries, Samuel Hartlib and Sir William Petty, much less Samuel Dell.

The book is cast in the form of a sustained argument with copious quotations and examples piled up carefully on one side. It looks suspiciously like the method of Christopher Hill, though from the opposite perspective. No doubt Tawney is dated and needs correction, but Tawney did it better, I think, and will still have to be read, despite the best efforts of this earnest book.

JOSEPH M. LEVINE
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JENNIFER LOACH. *Parliament and the Crown in the Reign of Mary Tudor*. (Oxford Historical Manuscripts.) New York: Clarendon Press of Oxford University Press. 1986. Pp. xii, 262. \$39.95.

Tudor parliaments, too often regarded as only forerunners of seventeenth-century rebellion, are now being examined in their own right and placed within a tradition of consensus, conciliation, and relative conservatism. Jennifer Loach provides the first comprehensive account of Marian parliaments and a counterargument to those who see them as dominated by conflict. This reign was not, she argues, a turning point, either toward the "seizing of the initiative" by the House of Commons or, as more recently suggested, toward the choice of Parliament rather than the battlefield as the arena for conflict between monarch and subjects.

Within its own confines, the argument of this book is persuasive, but the almost exclusive focus on Parliament gives a sometimes curious perspective on a tumultuous reign. On several central points Loach is convincing. She describes Mary's accession to the throne as a revolt of the counties against the center, a rally of the county Catholics on her behalf. The accession crisis thus gave Mary legitimate cause to believe that she should rely on counselors from outside the circle of professional politicians and that she had wide support for the restoration of Catholicism.

Loach argues that there were wide areas of agreement between crown and Parliament, especially on measures to combat the social and economic stresses of the period, but that Parliament also exercised an effective tacit resistance to measures that Mary wanted but knew were doomed to failure: the coronation of Philip and, perhaps, the exclusion of Elizabeth from the succession. On such constitutional issues the House of Commons would be vocal.

But the religious concerns of Members of Parliament were not overtly doctrinal: they concentrated on the legal and constitutional dilemmas posed by Mary's return to Catholicism: the law on treason, the treatment of property of the "exiles,"

and, overwhelmingly, the fate of previously secularized church lands. Members of Parliament were not ideologically or temperamentally rebellious, Loach emphasizes; it is difficult to draw convincing connections between any alleged "opposition group" in the Marian parliaments and the extra-parliamentary conspiracies. Therefore, these parliaments should not be used to illustrate a Whigish story of progressively increasing tensions between the crown and Parliament, which were only temporarily held in check during the next reign by the force of Elizabeth's character.

Many of these ideas are familiar, partly as a result of Loach's previous publications; it is good to see them grounded on careful documentation. For many, the book will be most valuable as institutional history: Loach is especially good, for example, on how the pressure of local interests on the M.P.'s made careful management of Parliament necessary to get crown business done at all. As a contribution to Tudor political history, the work is less revealing. The sizeable segment of the political classes in exile after Mary's accession renders questionable any inferences drawn from parliamentary activism (or lack thereof) about the mood of the "political nation" or its support for Mary's return to Catholicism.

Loach herself admits that in some respects Marian parliaments were atypical. There was an unusually high percentage of inexperienced members in the Commons, probably because of the absence of former M.P.'s gone into exile. The new bishops were selected more for spiritual qualities than for political talent, which threw the brunt of leadership in the House of Lords (already bereft of Edwardian Protestant temporal lords) onto the Henrician holdovers.

The queen's poor political sense emerges repeatedly. She not only neglected the political role of bishops in her appointments to the bench but also distrusted William Paget who was, as Philip better understood, the only one of her advisers really knowledgeable about foreign affairs. Rebutting the conventional argument that the size and factionalism of Mary's Privy Council was a sign of political disarray, Loach sees a healthy airing of diverse opinions. But these quarrels, spilling over into Parliament, surely poisoned the political atmosphere. If Mary's legacy was not (in the political sphere at least) a wholly negative one, this seems to owe more to the M.P.'s whom Loach has studied than to their monarch.

CAROLINE M. HIBBARD
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J. R. JONES. *Charles II: Royal Politician*. Boston: Allen and Unwin. 1987. Pp. 230. \$29.95.

Part of the dismantling of the Whig theory of seventeenth-century history has been to derogate the rise of Parliament as an inevitable consequence in English political history. If the claims of Parliament to constitutional supremacy now seem less compelling and its political role less certain than they did to Thomas Macaulay, the position of the monarchy deserves a different scrutiny also. J. P. Kenyon's *Sunderland* (1958) and J. Miller's *James II: A Study in Kinship* (1978) examined the role of the most inept of the Stuart kings along the lines of policies intended less to promote the cause of absolutism than to achieve practical objectives.

J. R. Jones's analysis of Charles II is similar to Miller's political biography of James II. But, whereas Miller's study is more or less an apology for the king, Jones's examination of Charles does not condone his subject. If anything, pragmatic statecraft is called into question so that Charles's policies after Clarendon's fall in 1667 seem so close to absolutist conceptions that the differences between the two political ideas seem not to matter. Jones analyzes the process by which Charles divided and conquered his ministers, his Parliaments, and the church, leaving a political vacuum at the time of his death. Unlike James II, Charles II sought to destroy his enemies rather than to control them and to recognize as an enemy anyone who threatened to place shackles on his authority. This seems less the politics of practical consequences and more the policy of a ruler unable to cope with the Cromwellian legacy of an England with a greater empire and civil service. To that extent the Tories—the greatest legacy of Caroline politics—seem strangely more attuned to Oliver than to Charles. In Charles's defense, writes Jones, England avoided another civil war and the perpetuation of local strife characteristic of Scotland during the same period. Do the ends really justify the means, especially when we know from hindsight what was to happen in 1688–89?

Students of the period will find no new factual information in this book, which is based entirely on well-known printed sources. Jones is forced to take for granted the gray areas of the period, such as the time of George Monck's decision for a restoration and Charles's own role in the process by which he regained his throne. Once again we are reminded of how an excellent biography of Clarendon could fill so many holes in the period. A close reading of the printed sources, Jones's study is a sharply etched portrait, constituting the best political study of Charles in print and useful as an insightful survey of the period.

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WILFRID R. PREST. *The Rise of the Barristers: A Social History of the English Bar, 1590–1640*. (Oxford Studies in Social History.) New York: Clarendon Press of Oxford University Press. 1986. Pp. xvi, 442. \$62.00.

This book can scarcely be commended too much for what it achieves in its own terms. It is a study in numerous aspects of the upper level of the English legal profession at the end of the sixteenth century and the beginning of the seventeenth. "Upper level" means those entitled to appear as advocates in most public courts, whether or not they had any practice or significant practice as advocates; "lower level" would signify attorneys who did legal paperwork. The familiar modern barrister-solicitor distinction is in some ways misleading, as the book amply explains, but it is the old system's recognizable descendent.

Wilfrid R. Prest has some interest in the concept of "professionalization," and in a sense his book is a profile of a "new profession." That sense should not be overdramatized, however. In the period under study, advocates (save for civil lawyers confined mainly to ecclesiastical and admiralty courts) were either serjeants-at-law or barristers. The former had, by royal appointment, the exclusive privilege of practice in one court, the Common Pleas. The latter class, ordinarily called "apprentices" (but with nothing like the ordinary associations of the word) existed before the late sixteenth century; the novelty was that non-serjeant advocates became much more numerous and more weighty in the profession as a whole. They did so on the basis of a great proliferation of litigation and other legal business and the much-increased relative importance of the courts to which they had access (the King's Bench, equity courts, and special-purpose courts such as the Star Chamber and Court of Wards.) For the sociology of "upper level" lawyers, serjeant-at-law *versus* barrister does not matter—serjeants were just a specialized and privileged subgroup, not coterminous with the most distinguished and well-employed lawyers.

This study is essentially sociological. That is, the lawyers are looked at apart from the internal workings of the law itself. This is a self-limitation, not a fault. At some points, the significance of the findings is hard to understand without more law than has been, or can easily be, penetrated. For example, one of the most interesting showings is that, in at least some courts (English-bill, primarily equity, courts are the ones scientifically studied for this purpose), a kind of favoritism or clientage relationship between lawyer and judge had a great deal to do with whether the lawyer would be recognized for the purpose of making motions. This practice was part of a larger tradition that the

impressions of anyone who has studied cases will confirm: the *de facto* monopolization of advocacy in the central courts by a relative handful of large-scale practitioners. How much the larger tradition depends on the more specific one is difficult to say without detailed anatomies of particular pieces and kinds of litigation: in order to further your client's course, was it helpful to enjoy the judge's favor in a way that seems at once scandalous by modern standards and all too harmonious with Tudor-Stuart ways of doing other forms of public business? In a telling quotation on page 72, a contemporary witness confirms (by way of complaining about it) the monopolization of "audience" that Prest demonstrates statistically. The complainant admits, however, that one would hardly expect litigants not to employ one of the chosen few if the case came to actual argument on the legal merits. (That you could have such a lawyer without an abnormal capacity to pay might be a democratizing grace from the litigant's point of view.)

In most aspects, the social history of the profession can be disengaged from the toils of the law. By combining random sampling and an impressive amount of illustrative material, Prest is able to discuss, among other things, social and educational origins; career patterns, income, and social mobility; the lawyers' religious and political affinities and their intellectual culture; their ethical standards and dilemmas and their sense of themselves. Conclusions, as is so often the case with good social history, tend to mute those sharp pictures of the past that circulate in our traditions and seem to make the past more savory than human affairs usually are—lawyers were not predominantly the younger sons of true gentry but drawn from a miscellaneous range of respectable ranks; some lawyers became rich, but many did not advance beyond the socioeconomic status of their parents. On these and many other matters, Prest's methods are carefully designed; his writing is cautious and clear, with abundant allowance for uncertainty and the complexity of interpretation.

CHARLES M. GRAY
University of Chicago

NICHOLAS TYACKE. *Anti-Calvinists: The Rise of English Arminianism, c. 1590–1640*. (Oxford Historical Monographs.) New York: Clarendon Press of Oxford University Press. 1987. Pp. viii, 305. \$57.00.

The thesis of *Anti-Calvinists*, familiar to readers of Nicholas Tyacke's 1973 essay in Conrad Russell's *Origins of the English Civil War*, now appears fully fleshed-out at an important juncture in the con-

tinuing reassessment of the religious history of pre-Civil War England. With so much emphasis placed of late on consensus in early Stuart church as well as state, Tyacke's book is a salutary reminder that the Civil War did not spring from a vacuum. According to Tyacke, the war is comprehensible in part as the outcome of very real and increasingly bitter conflict dividing the Church of England since the 1620s. The agents of division, innovators in doctrine and practice, were those who departed from the Calvinist theology that had united the English church for generations. These Arminian clergy and their lay patrons destroyed the middle ground of the pacific Jacobean church and forced the events of the 1640s.

While Tyacke draws the connection between sacraments and ceremonies on the one hand and theology on the other, the focus of his book is on doctrine. Calvinism, defined in terms of its rejection of human free will and its insistence on the perseverance of the elect, was the unifying bond of Elizabeth's church, welding puritan and conformist into a solid front against the Roman Antichrist. Threats to this doctrinal unity, first in the Cambridge disputes of the 1590s, were condemned in no uncertain terms by episcopal authority, and the teaching of the Elizabethan church was enshrined in the Lambeth Articles. The early Jacobean church followed suit, rooting Arminians out of universities and parishes. Tyacke recognizes the political advantages of James's Calvinism but argues that the king's predestinarianism was heartfelt, citing James's own writings as well as his choice of staunchly Calvinist delegates to the Synod of Dort in 1618.

Not until the 1620s was this pattern disrupted. Preferment of Arminians to episcopal status late in James's reign paved the way for their rise to exclusive power under his successor. Centered initially on Richard Neile and Durham House, the Arminian "party" seized the day at the York House Conference. They managed to ban teaching on predestination and foster instead the semi-pelagianism of Richard Montague (whose signal contribution to the cause may well have been his branding of the orthodox with the "odious name" of puritan) and the rigorously enforced ceremonialism of William Laud. Railed altars and stained glass threatened to replace sermon-centered Protestant tradition in the 1630s, and this "Arminian revolution" provided a focus for the religious opposition that would lead to the Long Parliament's actions in the next decade.

Tyacke is theologically sophisticated in treating the complexities of the predestinarian controversy. He illumines the subtleties of hypothetical universalism and acknowledges varieties within both Calvinism and Arminianism, but the funda-

mental doctrinal differences between the two remain clear. Taken together with the political maneuvering of Neile's network, they fully justify his two-party view. Despite recent claims to the contrary, Laud is certainly shown to have been either the Arminian we have always thought he was or a fool to have sponsored the likes of Montague.

While Tyacke has not supplanted the old categories of "puritan" and "Anglican," he has provided a more highly textured and convincingly nuanced picture of seventeenth-century religion than the received version has allowed. In this meticulous, lucidly written, cogently argued study, he has given us a new understanding of religion and the road to Civil War.

MARGO TODD
Vanderbilt University

ANN HUGHES. *Politics, Society, and Civil War in Warwickshire, 1620–1660*. (Cambridge Studies in Early Modern British History.) New York: Cambridge University Press. 1987. Pp. xvi, 392. \$49.50.

This is a valuable addition to the now lengthy list of civil war county studies. Much of the Warwickshire story will be familiar to readers of earlier works in the genre: the arable/pasture contrast; the temporary change in the composition of the governing elite; the heavy burden of war on both the gentry and the common people. But Ann Hughes notes some interesting features that are peculiar to Warwickshire. One is the brief ascendancy of Lord Brooke, a populist aristocrat even more energetic in the early months of the civil war than the Earl of Warwick in Essex. Hughes admirably dissects Brooke's patronage and protection of Puritan clergy, as well as his military leadership. But the most striking characteristic of Warwickshire, according to Hughes, was the absence of any cohesive gentry community: the county was not an effective social, political, or ecclesiastical unit. Hughes repeatedly snipes at the "county community" model favored by historians of other counties, rightly pointing out that the gentry's horizons were wider than many have supposed and their political behavior impossible to fit into a localist framework. In Warwickshire, she argues, the "county community" was a result rather than a precondition of the civil war: the gentry's bitter experiences between 1640 and 1660 produced an elite solidarity that had not existed earlier in the century.

Hughes is so anxious to discredit the notion of community, however, that she sometimes goes too far in ruling out localist feeling altogether. She is right that county politics did not always involve

resistance to central government and that there is no necessary equation between localism and political moderation. But, having sensibly reminded us of these points, Hughes makes some rather more controversial assertions. She will not accept as expressions of localism even the Warwickshire committee's resistance to orders to send local troops outside the county or their protests when their control of sequestrations was transferred to London. She rightly stresses that the moderate gentry had a program that was national, as well as local, in scope, but pays less attention to the fact that it was one that would have restored local power to local hands. Hughes cannot avoid noting that William Purefoy and his radical allies survived in Warwickshire through most of the 1650s because they controlled the local military forces: surely, they had that control because they had the confidence of the republican regimes at Westminster.

Even if Hughes sometimes gets carried away by her zeal to discredit conventional "county community" wisdom, this is still a fine study of Warwickshire politics during the revolution. Hughes's use of administrative records is impeccable, and she also does much to illuminate the religious life of the county from sources that include the little-known diaries of Thomas Dugard and Robert Beke. On one aspect of her critique of the "county community" school, Hughes is surely right: the English civil war can no longer be understood solely through studies of the gentry. Repeatedly, she reminds us that the common people were not mere mercenaries. They made choices and demonstrated them publicly—in wearing party colors at Coventry in 1642, for example, or, in one striking case, in a charivari against the royalist Lord Dunsmore. They had to be wooed, as Brooke realized, by preaching and propaganda, even sometimes by feasting and music. Hughes may not have proved that the Warwickshire gentry were not localists, but she certainly has succeeded in showing that the ordinary people of the shires cannot be left out of any analysis of the civil war.

DAVID UNDERDOWN
Yale University

HAROLD WEBER. *The Restoration Rake-Hero: Transformations in Sexual Understanding in Seventeenth-Century England*. Madison: University of Wisconsin Press. 1986. Pp. x, 253. \$27.50.

This book is an interesting attempt to subject the figure of the rake in seventeenth-century English drama to a Foucauldian analysis. Harold Weber argues that the emergence of the rake-hero in the

Restoration reflects a major transformation in cultural conceptions of the sexual. By analyzing the rake's career, he undertakes to illuminate the transition from a Jacobean and Elizabethan discourse on sex, which was dominated by a rhetoric of "demonic sexuality," to late-seventeenth-century secular understandings of the erotic characterized by a multitude of sexual languages, of which the demonic was only one. Indeed, Weber argues, the rake's career on the Restoration stage "initiates the modern 'discourse'" through which English culture attempted to transfer control of sexuality from the divine to the secular world and, at the same time, to transform the relationship between sexuality and individual identity (p. 10).

Weber sees the changes in sexual discourse dividing the courts of James I and Charles II as a consequence largely of a decline in belief in a divine hierarchy that presupposed a fundamental link between the sexual and the demonic, a decline he associates historically with the decrease in witchcraft prosecutions after 1650. This, he holds, is the point at which "the rake replaces the witch as a primary image of social disorder and erotic compulsion" (p. 45). The first chapter of his book illustrates this shift skillfully through a comparison of John Webster's *White Devil* (1612) and Thomas Otway's *Venice Preserv'd* (1682), both plays that exploit a rhetoric of demonic sexuality but that differ markedly in the consistency and intensity with which they elaborate that language.

The three chapters that follow delineate the diverse sexual discourse generated by a secular culture that saw sexuality not as alien and threatening to the self but as a necessary and integral part of personal identity. For Weber, this discourse manifested itself most prominently in a complex taxonomy of rakish behavior, the transformations of which he charts through discussion of a variety of types of rake: the "Hobbesian Libertine-Rake," the "Philosophical Libertine," and the "Female Libertine." The book's conclusion, which examines the proliferation of sexual discourses other than the demonic that emerge in the eighteenth century in genres other than the comedy of manners, includes illuminating if somewhat inconclusive discussions of libertinism in *Moll Flanders*, *The Beggar's Opera*, and *Fanny Hill*.

Weber's strengths are as a reader rather than as a historian or a literary theorist. His textual discussions are rich both in range and detail, and they are consistently well-written, as are his eminently balanced summaries of secondary historical material. As these summaries provide the sole contextual underpinning of his book, however, they remain too general and too tentative to be thoroughly persuasive. Weber's understanding of the

relationship between discursive and historical contexts, moreover, remains quite vague, and at times he seems to misunderstand Foucault's concept of sexuality as discourse, accommodating it to more traditional forms of cultural analysis or intellectual history. There is some refreshing clarity about historical contradictions: the shift from a demonic to a secular and liberal discourse on sex, Weber cautions, does not imply cultural liberation from fears of the erotic so much as a reorganization of those fears; nor, he shows, can the emergence of actresses on the English stage be understood simply as progress for women when, within discourse, that historical change worked both to elevate and degrade them. But, too often, Weber himself lapses into the rhetoric of sexual appreciation he undertakes to analyze, a tendency particularly marked in his reading of *The Country-Wife* (where one notes, among other things, the lack of any attempt to address Eve Kosofsky Sedgwick's analysis of the same play in *Between Men: English Literature and Male Homosexual Desire* [1985]) and in his reference to new *representations* as new "appreciations" of the erotic (p. 47). These lapses generate their own contradictions within his book, which finally remains unclear about whether it wants to posit sexuality as an essential or a purely representational category. Weber treats monogamy and heterosexuality, as well as the fear of sexuality (especially female sexuality) that accompanies them in the West, as matters of "fundamental social necessity" (p. 5); he even invokes Freud as a cultural authority on the relation of sexuality to social order (pp. 5–6). But if the point is that sexuality is constituted *within* discourse as an object of fear and desire, then it becomes problematic also to write of it as existing *prior* to representation and as generating anxiety and ambivalence, which is then "expressed" discursively. Weber's book is a welcome contribution to the study of Restoration drama, where pitifully little of theoretical interest has been produced, but on these matters of critical importance it fails to push its analyses far enough.

ELLEN POLLAK
Harvard University

LEONORE DAVIDOFF and CATHERINE HALL. *Family Fortunes: Men and Women of the English Middle Class, 1780–1850*. (Women in Culture and Society Series.) Chicago: University of Chicago Press. 1987. Pp. 576. \$35.00.

The authors have set themselves the enormously ambitious task of charting the religious, economic, and family history of the English provincial middle class in the period between Mary Wollstone-

craft's *Vindication of the Rights of Women* (1792) and John Stuart Mill and Harriet Taylor's "The Enfranchisement of Women" (1851). Leonore Davidoff and Catherine Hall exploit a vast range of primary documents focusing on industrial Birmingham and the agricultural counties of Essex and Suffolk to paint a remarkably detailed picture of the public and private lives of banking, trading, farming, and professional families. The central argument advanced is that a new sexual division of labor underpinned the successes of the early nineteenth-century middle class, a class spurred on not simply by pursuit of profits but by a dream of domestic bliss.

The moral pretensions of the English middle class have long been recognized as grounded in evangelical Protestantism, and this study accordingly begins with an analysis of religion's role in shaping new class and gender identities. Davidoff and Hall, while noting the irony that a member of the gentry like William Cowper and a single woman like Hannah More should have helped crystallize the new ideal of Christian, middle-class married life, stress the social advantages offered men and women by church and chapel. Religion provided women a "space" in which they could wield informal powers; it presented politically ambitious men with an apprenticeship in public life and the commercially active with an invaluable network of contacts. Most important of all, evangelicals—in denigrating the mores of both idle aristocrats and dissolute workers—hammered out the doctrine of the proper spheres, which held that respectable women were to be passive dependents and serious men dependable providers. Few would argue with Davidoff and Hall's assertion that the language of the emerging middle class was gendered. But, if others have recognized that Victorian women were not alone in being subjected to such ideological constraints, the authors provide the fullest account to date of the ways in which the symbolic importance of sexual difference colored the discussion of almost every aspect of social life.

Such an ideology obviously had its inherent contradictions. What went on in the minds of purportedly independent businessmen when they found that they could only survive by falling back on the support of wives, family, and friends? Part 2 of the study—the most interesting section of the book—closely pursues the various ways in which a concern for and an exploitation of kith and kin underlay English economic life. A manufacturer looking for partners, a father establishing a trust, a banker seeking creditors, a tradesman recruiting help, a youth seeking employment thought first of turning to relatives for help. And—as Davidoff and Hall brilliantly demonstrate—an essential

"hidden investment" in middle-class economic prosperity was made by purportedly passive middle-class women. Wives, mothers, and sisters continued—despite the increased separation of home and work and the barriers thrown up against women's economic activity that led to further structured inequality—to provide their proudly individualistic male relatives with invaluable amounts of capital and unpaid labor.

Middle-class women appear to have silently accepted the narrowing of their options, but they too were forced to grapple with the tensions inherent in an ideology that declared their spiritual equality yet required their social subordination. Davidoff and Hall carefully avoid condemning women for being deflected into domestic tasks that with hindsight appear isolated and trivial. What the authors do demonstrate is that even those who embraced the doctrine of female fragility were at the same time expected to cope with the enormous burdens of bearing large numbers of children and managing increasingly complicated households. *Family Fortunes*, in skillfully dissecting the tensions inherent in the domestic dreams of the middle class, thus sets the stage for the emergence of the late nineteenth-century women's movement.

The strength of *Family Fortunes* results from its encyclopedic portrayal of the lives of provincial middle-class families. If the book has a weakness, it is that the richness of detail is at times overwhelming. The reader is presented with so many vignettes demonstrating the myriad of ways in which gender preoccupations colored the discussion of religion, politics, literature, business, education, marriage, housing, and even gardening that the inattentive may at times not see the forest for the trees. But the conscientious reader can only welcome a study that rewards repeated readings.

ANGUS MCLAREN
University of Victoria

PAT HUDSON. *The Genesis of Industrial Capital: A Study of the West Riding Wool Textile Industry, c. 1750–1850*. New York: Cambridge University Press. 1986. Pp. xx, 345.

Many historians and economists have long believed that the study of the financing of industry, and particularly the means of accumulation of industrial capital, is a vital tool in the study of the whole process of industrialization, even likely to prove more useful than any other. Particularly in the British context, it has seemed that this could be true in understanding the growth of the great industrial regions in the eighteenth and nineteenth centuries. Some ambitious team projects

that were launched to attack this issue failed for accidental reasons to achieve their full purpose. It is all the more remarkable that this single-handed work constitutes an outstanding piece of research and has confronted the subject in all its complexity. Pat Hudson is always trying to reach conclusions but not by easy, facile, or trendy routes.

To begin with, there is a detailed discussion of the work of economists and historians in developing the study of capital accumulation. Recent emphasis on the great importance of circulating capital during British industrialization is among the many topics and interpretations discussed. Then the West Riding wool textile industry is looked at between the mid-eighteenth and the mid-nineteenth centuries, and, although Hudson properly points out the extensive work of previous historians of the industry, her own review and reappraisal is a penetrating and sometimes novel one. The distinction between the wool and worsted branches is reexamined, and the main expansion of the worsted branch is shown to date from the second half of the eighteenth century. The suitability of the jenny for spinning in the woolen branch, the suitability of the powered frame or throstle for worsted yarn production, and the earlier applicability of power to worsted weaving are explained, with their consequences for capital investment. In the woolen branch the development of mills not only for fulling but also for scribbling—mills often owned by partnerships of small clothiers—allowed the clothiers to continue as independent (but not putting out) producers in spinning and weaving far into the nineteenth century. Next, “primary accumulation” of capital in the region is looked at with a particular focus on the recent massive number of works on “proto-industrialization” to which Hudson has been a main contributor, although she has not been uncritical or a full convert to the theory. There is an excellent discussion of the quality of land and the nature of landholding and inheritance in areas and communities predisposed to production of the different types of wool textiles by direct domestic production or by a putting-out system. The central sections of the book are marked by depth of detail and closeness of argument and need to be read carefully to be properly appreciated. Part 3, “The Web of Credit,” constitutes to date the best examination by far of the role of credit in any major section of Britain’s industrializing economy. It shows how in different branches, for instance, in the supply of wool or in the trade in wool products, there were remarkable changes over time, particularly in the period over which credit was extended. These changes are related to a large number of considerations, by no means solely confined to technology or to the geographical

span of trade. This substantial section of the book has to be regarded as having changed the state of the art in this aspect of the economic history of the period.

Hudson ends her review of the evidence with an examination of the finance, both internal and external, of the industry. She opens with an analysis of the role of the attorney, a subject already pioneered by B. L. Anderson and others, but Hudson shows the importance of attorneys in this region. She incidentally stresses a point made elsewhere in the book about the great importance of the mortgage in allowing the raising of capital, even by people of modest means, which enabled them to participate in the early stages of the development of industry, because industrialization required increasing investment. Hudson demonstrates that the bankers of the region had very close connections to the textile industry; indeed, remarkably, they were often directly involved in both manufacturing and banking. Overdrafts to textile firms were substantial, and money was lent in practice for long periods, even if nominally on current account. Critical analyses of the contributions of British banks to the capital requirements of industry, already queried in recent years, will have to be further revised. It is interesting that, though François Crouzet, whose seminal work influenced all subsequent studies on capital formation, contributes a generous foreword to the volume, Hudson is a little dubious that the evidence from West Riding fits in with Crouzet’s views of the plowback of profits as the dominant source, rather than just a very important source, of funds for capital formation in this period. Just occasionally some predisposition to a Marxist interpretation seems to surface, but the examination of the evidence seems to be thoroughly objective, and the outcome is a pioneering work of real importance.

J. R. HARRIS

University of Birmingham

JOHN A. TILLEY. *The British Navy and the American Revolution*. (Studies in Maritime History.) Columbia: University of South Carolina Press. 1987. Pp. xviii, 332. \$24.95.

A modern book devoted to the British navy in the war of the American revolution has long been needed. The last such study, *The British Navy in Adversity* by W. M. James, is more than sixty years old. Unfortunately, John A. Tilley’s work does not fill the need.

Although the American revolution was truly a global war, especially at sea, Tilley limits his account to the North American station of the Royal

Navy. Even Sir George Rodney's victory over the Comte de Grasse in the battle of the Saints (called "Saintes" by Tilley) gets slight mention, although it certainly had as much to do with the final Treaty of Versailles as de Grasse's defeat of Thomas Graves in the battle of the Chesapeake. Pierre Suffren, perhaps the toughest opponent the Royal Navy met in the eighteenth century, makes it into one parenthetical phrase, while Trincomalee and Cuddalore—and indeed all operations in Indian waters—are entirely ignored. Even in North America, the Saint Lawrence River might not have existed. The fresh-water battle of Valcour Island (perhaps the most important naval action of the war) does have a short paragraph, but Tilley gives no indication that he appreciates the difficulties the Royal Navy faced in building a fleet that could take Lake Champlain away from Benedict Arnold.

Tilley has done impressive digging in primary sources here and abroad. It is a pity he did not seek guidance from some standard secondary accounts, such as Michael Lewis's *History of the British Navy* (1957), which does a clearer, more comprehensive job of covering the war in sixteen pages than Tilley does in twenty times as many. (He should also have checked the titles before he sent his bibliographical essay to the printer.) Tilley is best when telling about individual station commanders, for example, Samuel Graves, Molyneux Shulldham, Richard Howe, James Gambier, and Marriot Arbuthnot, though his conclusions about most of them are more charitable than the evidence he presents seems to warrant.

Tilley's writing style may be characterized by the words he uses in describing the style of Sir Julian Corbett: "sometimes incomprehensibly verbose" (p. 315). No noun, no verb sails without a convoy of adjectives and adverbs, many of them ill-suited for their task. Can even Sir Edward Hawke suffer "a consummate lack of energy" (p. 132)? What is "a disciplined melee" (p. 257)? There are so many intensifiers that one does not know what is truly important. Greater economy of words would not only have made the book clearer but would also have left room for the subjects the title implies will be covered.

Finally, although readers may learn something from this book, I caution them to skip chapter 1. It will irritate scholars of the American revolution, mislead those who are not, and add nothing to anyone's knowledge of the Royal Navy in the War for Independence. *The British Navy and the American Revolution* could have been a good book, for Tilley obviously loves his subject, but the publisher neglected to send the manuscript out to readers who know something about the topic beyond the mere use of nautical terms. Publication of this book should forestall no one who has contem-

plated writing a book or dissertation about the Royal Navy between 1775 and 1783.

NEIL R. STOUT

University of Vermont

JOHN SAVILLE. *1848: The British State and the Chartist Movement*. New York: Cambridge University Press. 1987. Pp. ix, 310. \$44.50.

As John Saville notes, the year 1848 is often treated as an example of a revolutionary moment whose impulse by-passed Britain. In contrast Saville locates British domestic politics within the triangle of revolutionary Paris, insurgent Ireland, and a revitalized Chartist movement that manifested itself most strikingly in London and in certain districts of the industrial north. This highly suggestive study fully plays out themes that have concerned the author ever since the publication of his article "Chartism in the Year of Revolution, 1848" (*Modern Quarterly* 8 [1952–53]). This book is not a history of Chartism in the year of European revolution, however, but rather a carefully researched assessment of "those directing the various organs of state power, and those in Britain and Ireland who were regarded as actually or potentially dangerous to the stability of the established order" (p. 2). Most originally, the book offers a meticulously drawn picture of the authority of the British state during this crucial year.

In 1848 Lord John Russell's government faced a triple threat: the example of the February revolution in France, the fury of the Irish people suffering the catastrophic effects of famine, and the Chartist movement, which at last moved into an alliance with sections of the Irish nationalist movement. Saville is impressed with the coordinated governmental response to this challenge and with the ultimate security of the state. He convincingly argues that this security was based on the coherent administrative machinery of local and central government and the coercive force at the disposal of those directing the defense of the established social and political order. He also stresses the collective experience and competence of those in charge at both the national and local levels, along with those in military command. Saville regards the post-1832 state as essentially "bourgeois," with local authority increasingly in the hands of the middle class. National government remained under the control of traditional aristocratic rulers who prudently pursued social and economic policies broadly consonant with the interests of the bourgeoisie. The true significance of 1848, therefore, lies less in the coming together of forces striving for democratic change than in the realization of shared interests on the part of Britain's

propertied classes. The most arresting expression of this coalescence of interests was the willingness of members of the middle class to serve as special constables in April 1848—"a *levée en masse* of quite remarkable proportions" (p. 27).

These broad interpretive themes are well illustrated in chapter 6, "Days of Judgement," which superbly details the operation of the law in 1848 in England and Ireland. The central government generally refused to accede to local magistrates' requests for the arrest of Chartist leaders until the late spring, when they might be sure of convictions. The results of the trials of Chartists confirmed the wisdom of this strategy. Liberal notions of the constitutional rights of free assembly and speech were universally denied by judges and by the swift verdicts of guilty returned by middle-class jurors. The situation in Ireland was different; here, juries were packed with loyal Protestant subjects. Furthermore, the government tried Irish leaders, most notably John Mitchel, under the recently passed Crown and Government Security Act, which made convictions for treason more certain by reducing the penalty from death to various terms of transportation or imprisonment, and which included "open and advised speaking" as a category under which prosecution could be brought.

The book's final chapter addresses the difficult question of the roots of mid-Victorian working-class reformism. Saville argues that, having been finally broken by the physical force of the state, Chartism was almost completely obliterated from public consciousness, "submerged . . . beneath layers of false understanding and denigration" (p. 202). He gives less explanatory weight than some recent scholars, particularly Gareth Stedman Jones, to the more moderate posture of the British state in the mid-1840s, arguing that government concessions were merely the other side of the coin to state repression. He stresses the growing influence of middle-class ideas that served to confuse the development of a counterideology among working people. Although the author sometimes overstates his case, this excellent study confirms Saville's distinction as a clear-sighted and ever provocative historian.

JAMES EPSTEIN
Vanderbilt University

GREGORY CLAEYS. *Machinery, Money, and the Millennium: From Moral Economy to Socialism, 1815–1860*. Princeton: Princeton University Press. 1987. Pp. xxx, 245. \$35.00.

This book is a valuable contribution to our understanding of early socialist thought in Britain.

Gregory Claeys focuses on that group of writers—Robert Owen, George Mudie, William Thompson, John Gray, and those who wrote in the tradition established by them—who between 1815 and 1850 tried, in light of their "common allegiance to the school of socialism founded by Owen, to reconceptualize economic thinking as they understood it" (p. xxi). These thinkers were linked partly by their shared protest against postwar distress but also (and more importantly) by a new vision of economic order in which spreading mechanization would create a plenty "enjoyed by all without the severe curtailing of needs" (p. xxii). This new vision, Claeys argues, represented a "sharp break" from the "just price and fair wage tradition of moral economy" (p. xxvii) prevalent in earlier forms of British radical thought. Moreover, by embracing new modes of industrial production, Owen and his disciples offered their age an alternative to the standpoint of Ricardian political economists. An "acquiescence in some fundamental assumptions of commercial and manufacturing society assisted Owen's assertion that his plans embodied a more practical, more complete analysis of society based on 'much profound study of the whole circle of political economy'" (p. 50).

Claeys persuasively challenges the Marxist account of Owenism as a "utopian socialist" retreat from an emerging industrial world into model rural communities. He also provides a useful corrective to the still familiar view that British socialists have never displayed the philosophical rigor of their Continental counterparts. For these reasons, his is a book no students of Owen, British intellectual history, and socialism can afford to ignore.

But there are some problems with Claeys's alternative account of the historical reputation of Owen and his disciples. One is an indecisiveness in his analysis of the main practical bearing of Owenism. In his introduction and conclusion, Claeys writes that his goal is to play down "the more conventional emphasis on Owenism as an essentially agrarian and communitarian movement," and stress "instead the extent to which it came to share in the Victorian vision of commercial and industrial progress" (p. xxii). The main chapters of his book, however, regularly highlight the extent to which *both* conceptions of socialism coexisted within Owenism from its origins to its demise.

This more subtle analysis of an unresolved tension within Owenism emerges from Claeys's close and thoughtful reading of Owenite writings. Yet here another difficulty surfaces. A careful explication of texts (one of the great strengths of this book) surely is essential in showing that the economic ideas of Owen and his followers "were more complex and elaborated in a greater degree than

previous interpreters have allowed" (p. 66). But it is arguable that the real historical question about Owen is why previous interpreters—including contemporaries sympathetic with his goals—missed the deeper aspects of his thought. We need answers to questions that Claeys raises but leaves largely unexplored. Why did many Owenites become "completely enmired" in "endless public debates on religion and sexual relations" that "virtually crippled" their movements in some areas (p. 152)? Why did Owenism fail in its efforts to dethrone Ricardian political economy as the reigning "science of wealth" in Britain? It appears that demonstrating the underestimated intellectual depth of Owen's thinking only takes us so far in grasping the actual historical fate of Owenism. We need to know more about the cultural organization of British intellectual life during Owen's lifetime.

Claeys ultimately skirts these issues by trying to assign (in Chapter 7) a central role to Owen's ideas in forging the critical weapons used by Engels and Marx. Yet this argument displays the ambiguity that plagues most studies of influence. It may be possible to show that Engels's early "Outlines of a Critique of Political Economy" (1843) owed more to the pre-Marxian socialism of Owen than has generally been conceded. But, since Engels himself apparently never saw (or admitted to) this connection, it also is possible Claeys overestimates the significance of Owenite ideas. Or, put another way, since making the concept of "influence" meaningful requires historians to distinguish it from more straightforward concepts such as adoption or paraphrase, we have to leave open the possibility that the inner connection Claeys uncovers is no more than a coincidence.

Much of the history of Owenism, then, still remains to be written. Even with its shortcomings, however, this book will provide an important point of departure for that work.

RICHARD F. TEICHGRAEBER III
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J. V. BECKETT. *The Aristocracy in England, 1660–1914*. New York: Basil Blackwell. 1986. Pp. 512. \$45.00.

This study of the English aristocracy from the Restoration to the Great War is divided into three parts: part 1 is concerned with the creation and nature of the aristocratic order—who the aristocrats were and how they got there; part 2 deals with their relationship to the economy, specifically their role in the agricultural revolution, in industrial development, in communications and in urbanization; part 3 considers the role they played in local and national government.

J. V. Beckett's thesis is straightforward; between 1660 and 1914 England was controlled rather well by a tiny, powerful, confident elite, a group more or less closed to outsiders. As D. Rubinstein and Lawrence and Jeanne Stone have demonstrated and Beckett confirms, openness was a well-rehearsed myth.

The basis of the elite's power and privilege was property, the key to status and position in English society. Those fortunate enough, however, to obtain the right amount of land in the right place by whatever means did not immediately (or even ultimately) arrive. They lacked pedigree, the confidence to be superior. To acquire it took time, the right education, and frequently the right marriage. Successful socialization into attitudes of confident superiority, Beckett tells us, took at least two generations. Confidence was enhanced by the absence of effective challenge. For centuries, the English have been marvelously tolerant of their aristocracy. There was no revolution in the late eighteenth century, of course, and subsequently the rising middle class proved deferential to the point of obsequiousness. This was a state of affairs Walter Bagehot greatly admired. It kept oligarchy at bay. Deference was a result in large measure of a widespread belief that the aristocracy was born and bred to govern—and govern judiciously (independent income ensured impartial judgment). Hegemonic influence in this regard in the age of the new imperialism and the golden age of the public school was considerable. To rule was decent; to make money, as distinct from to have it, indecent. To serve society at home or overseas was the highest civic honor. Noblesse oblige!

Beckett finds much to admire in the contributions of the aristocracy to English society between 1660 and 1914. State, locality, and family, he argues, all benefited from the sensible capitalism of this group. They were a force for good, despite being frequently underestimated and insufficiently appreciated. In the main they both conserved their estates with taste and developed them with good sense. They played the role of rentier-director with considerable acumen, leading their management teams nationwide with aplomb and energy. They proved to be admirable developers providing risk capital for enclosure, drainage, and farm building, and they took the initiative in mineral exploitation and communication schemes.

In Beckett's view they have been treated harshly and wrongly denied a good press. Too frequently they have been viewed as butterfly dilettantes, ultra conservative entrepreneurs or self-interested upper-class Luddites with amateurish and restricted attitudes toward beneficial change. Beckett, however, will only allow their critics a grain of truth. In general, he asserts, the English aristocracy

crats played a strongly positive role in the preservation and development of their estates. Better still, since it was allegedly as much altruistic as self-interested, the principle of the public good was always firmly in their minds. In general, they showed themselves socially responsible and civic-minded both locally and nationally. The only area in which they failed to play a leading role in English society was in industrialization, largely because the limits of their enterprise usually stopped at the boundaries of their estates. This is, of course, rather a large failure and arguably one that illustrates at one and the same time their entrepreneurial limitations and myopic self-interest.

Beckett further argues that their much-publicized and deprecated indebtedness has been exaggerated and, in any case, was not necessarily detrimental to the economy. On the one hand, duty ensured investment on estates beyond the cautious limits of hard-nosed business sense, and, on the other, an expensive life style was expected of them, and a shift of emphasis to facilitate economic development would merely have harmed their prestige. Little wonder then that their sense of duty was so well developed!

This concept of duty, as we have seen, extended beyond conspicuous consumption in the interest of image to leadership of both local and national society. At a local level, their splendid houses were not merely homes but centers of consumption and employment, of entertainment and community cohesion. This was a cohesion, suggests Beckett, founded on benevolent paternalism, both formal and informal, through the administration of justice, the provision of alms, the encouragement of local entertainment, and the support of church, schools, and cottages. These mechanisms, of course, could be potent instruments of social control. There is an element of "merrie England" in Beckett's account of aristocratic paternalism, and the seeker after balance would do well to read, for example, Bob Bushaway's *By Rite* for a view of this paternalism from underneath.

At a national level the aristocracy was expected to govern, and it did. Often, Beckett reminds us, at considerable personal cost. Life in London was expensive, and neither Members of Parliament nor peers were paid. There must have been great satisfaction in such sacrifice. After 1688 the aristocracy came to control every aspect of government, both executive and legislative, which remained broadly the situation until 1914 despite vigorous anti-aristocratic campaigns in the 1790s, the Reform Bill of 1832, and harsh enforcement of the game laws. The longevity of the aristocrats' power was certainly remarkable. According to Beckett, this was because they never became

anachronistic. Their contribution was invariably positive if not perfect. Nevertheless, the second half of the nineteenth century saw their grip lessening on English society.

Increasingly as Victoria's reign progressed, military incompetence, parliamentary and electoral reforms, financial difficulties, successful legislative challenges from farmers and tenants, the middle-class invasion of Commons and cabinet, the creation of industrial peers, and the gradual rise of the Labour party saw the aristocracy's hold on power weaken. By the end of the Great War it had gracefully declined into a position of decorative splendor. English tolerance was at last exhausted. In political terms, at any rate, the aristocrats had had their day. The story of their dominion is certainly a fascinating one, and Beckett has written a forceful, detailed, and very readable defense of their role in English society.

J. A. MANGAN

Jordanhill College

GARY W. COX. *The Efficient Secret: The Cabinet and the Development of Political Parties in Victorian England.* (Political Economy of Institutions and Decisions.) New York: Cambridge University Press. 1987. Pp. xiv, 187. \$32.50.

The title of this study leads one to expect another look at the political order in Westminster. And, indeed, more than half of the book is devoted to an examination of the emergence of disciplined parties in the House of Commons. Gary W. Cox argues that this feature of modern British politics arose primarily as a result of what Walter Bagehot called "the efficient secret" of the English constitution: namely, the assumption by the cabinet of control over the legislative agenda of Parliament. In order to affect the legislative action of the Commons, M.P.'s had to unite themselves to one or other of the two major parliamentary parties, the larger of which would form the government.

Through a mixture of quantitative analysis and attention to chronology, Cox eliminates many of the hypotheses offered to account for the strengthening of party voting in the Commons. The cardinal fact he establishes is that this behavior emerged as the predominant pattern between 1857 and 1868, the decade that preceded enactment of the Second Reform Bill. That dating deals a blow to explanations for the rise of party discipline that concentrate on the expansion of the electorate through the Reform Acts or on the subsequent development of party organization.

Cox moves onto fresher, more interesting ground when he turns to examine the emergence of firm party voting outside Parliament among the

electorate. The statistical base that he uses for this part of his study consists of returns from the double-member constituencies that dominated the English electoral map until 1885 and lingered in much reduced numbers until 1918. The returns from these constituencies, where electors could split their votes between rival parties, provide splendid and hitherto underexploited evidence of the rise in partisan voting among the electorate. Cox dissects the evidence with just the right combination of quantifying subtlety and historical acumen.

The most fascinating fact he establishes is, again, that here in the constituencies, as in Parliament, loyal partisan voting emerged as the predominant pattern of behavior between 1857 and 1868. This simultaneity cannot be mere coincidence. Cox demonstrates that the party loyalty of an M.P. as reflected in his votes in the House of Commons and the party loyalty of his constituents at election time on the whole faithfully reflected each other.

He accounts for this phenomenon among the electorate as an extension of the effects of Bagehot's "efficient secret." But his explanation leaves room for doubt. The erosion of the legislative power of individual M.P.'s occurred from the 1830s through the 1850s. The last decade of this process coincided with the lessening of party loyalties in the Commons and the country that followed the disruption of the Conservative party over repeal of the Corn Laws in 1846. This contretemps in Cox's chronology should stimulate reflection. He demonstrates the rise to dominance of party loyalties from 1857 to 1868, a decade earlier than historians have usually supposed. But perhaps the explanation for this insistent partisanship, at least so far as the electorate is concerned, rests with another "secret," to which John Vincent pointed in *The Formation of the British Liberal Party* (1966): the explosive growth of the popular press and hence of popular political loyalties after the repeal of the stamp tax in 1855. There is still room to wonder.

PETER T. MARSH
Syracuse University

ROSEMARY ASHTON. *Little Germany: Exile and Asylum in Victorian England*. New York: Oxford University Press. 1986. Pp. xiv, 304. \$34.95.

In this gracefully written study, Rosemary Ashton describes many aspects of the lives of German exiles of 1848 who settled in Britain. Mid-century exiles to England from among many other nationalities have found their historians, but the heterogeneous Germans—who were overshadowed by

their most famous member, Karl Marx—have not been taken into account before this rich, colorful, clear, and often witty book was written.

Chapters include "The Communist Clerks": Friedrich Engels, Georg Weerth, and Ferdinand Freiligrath; "The Communist Intellectuals: Marx and His Party"; and "The Bourgeois Refugees," which features Arnold Ruge and Gottfried Kinkel and includes numerous journalists, doctors, artists, and teachers. There is also a chapter entitled "The Women of the Exile," which concentrates on Johanna Kinkel, the self-styled "mother of immigrants," and the unique Malwida von Meysenbug. A chapter on the proletariat and the lumpenproletariat takes up a mere 19 pages out of a total of 224 in the text, which shows how Ashton emphasizes those German exiles who were middle class. And, even among them, those who wrote gain disproportionate attention over those who were doctors, artists, and musicians.

Research is thorough from the scholarship in literature rather than from that in social history. The book reveals the differences in method and emphasis between these two disciplines. Social historians would probably want to dwell on such topics as the number of German exiles in comparison to other national groups, living standards, patterns of occupation, places of origin in Germany, and the religious and social differences that the exiles brought with them. Instead, Ashton concentrates on what the Germans observed in their publications, correspondence, and memoirs. Contacts with British literary figures are always stressed. Nothing is drawn from the Public Records Office or from working-class newspapers, but there are at least thirty references to Charles Dickens and his writings. The reader often learns less of what the exiles did than of what they wrote.

Nonetheless, many worthwhile insights emerge. One of the main purposes of the book is to view Victorian England through German eyes. Although most of the Germans admired England's freedoms, they were keenly aware of social, religious, and sexual prejudices. After all, most of the exiles had been agnostic rebels in the German states. Ashton describes the difficult adjustments they had to make. It is sad that many became chronic borrowers; only Engels was a chronic lender. Teachers had the goal of finding secure "berths," but in most cases they came to resemble the insecure contemporary academic "gypsies" who race here and there to give lectures and lessons. Musicians and artists had a hard time of it, but workers faced the most desperate competition of all.

The author shows how bitter pettiness marked exiles' politics. Of course, Marx was the most trenchant in this regard. His observations, often

wickedly humorous, permeate this study and are not confined to the substantial portions of chapters assigned to him.

This review ends with a quibble. The title seems ill fitting. "Little Germany" would seem to refer to a distinct district, such as "Little Italy" in New York City. These Germans of 1848 were scattered geographically, which in part reflects their social diversity. Perhaps "Aspects of German Exile and Asylum in Victorian England" would be a more appropriate title for this fine book.

HENRY WEISSER
Colorado State University

EDWARD NORMAN. *The Victorian Christian Socialists*. New York: Cambridge University Press. 1987. Pp. 201. \$34.50.

A few committed middle-class and upper-class Christians witnessed the ascendancy of capitalism in British society during the age of Victoria. They were repelled by the "steam-age mentality" of the new competitive system, not to mention the innovative "Manchester Radicals" (as they insisted on labeling the capitalist masters and theorists of political economy). These Christians, at least in the mid-nineteenth century, were for the most part "Tory radicals," imbued with rural romanticism and a sense of admiration for the landed gentry. The Chartist revolt in 1848 was the starting point of their taking flight from the received social attitudes of the Christians of their day. Eventually, they called themselves "Christian Socialists," and they were mortified by the failure of the church to speak of anything but the expiation of individual sin. The Nonconformist Churches spoke seriously about sin; the Christian Socialists were Anglican, at first.

This book is replete with the ideas of Frederick Denison Maurice, as indeed it should be. It is a study of the thinking of Maurice, Charles Kingsley, John Malcolm Ludlow, Thomas Hughes, Stewart Headlam, John Ruskin, Hugh Price Hughes, and Brooke Foss Westcott; but Maurice was the progenitor of almost all of the philosophy of the Christian Socialists. The reflections of Maurice resound, from the early era of Christian Socialism, 1848 to 1854, through the interim period, to the revival years, 1877 to World War I, the years of the Guild of St. Matthew (1877: Stewart Headlam), the Christian Social Union (1889: Westcott, Charles Gore, and Henry Scott Holland), the Christian Socialist League (1894: Nonconformist) and Church Socialist League (1906: political in flavor and Guild Socialist in disposition). Dislike for Benthamism, the horror of systems, the rejection of the "disease" of com-

petition, the adherence to associationism, the co-operative principle, the smallholding ideal, and adult education—all these were in Maurice's mind and were in Christian Socialism, too. But Christian Socialists of all sorts valued Maurice for his elevated doctrine and concern for humanity.

There is no way to summarize this thoughtful and thought-provoking book. Norman has a rare respect for previous scholars; he places the Christian Socialists where he thinks they were in religious and social history; the book is civilized, tempered, and full of empathy to thinkers in the nineteenth century like Kingsley, whose racism, anti-Catholic feeling, and nationalism are obviously at odds with the author's own values. Ludlow, the Anglo-Frenchman, who died at the age of ninety-one in 1911, was present in the early period of Christian Socialist history and joined the Christian Socialist Union in 1889, was the main protagonist linking the cooperative movement and trade union struggle throughout these years. Hughes was "unperplexed with doubts" (p. 80) and kept the faith with rural life, Rugby School, sport, "muscular Christianity" and "democracy without Jacobinism" (p. 84), in line with his popular novel, *Tom Brown's Schooldays* (1857). Headlam, the first of what I call "Sacramental" Socialists, to the left of the Fabians, whom he joined in 1886, had problems with the hierarchy. "Priesthood binds me to Radicalism," he said (p. 119); the bishop of London prohibited Headlam from preaching. John Ruskin is the one figure who is uncertain in this panoply of Christian Socialists. Ruskin was not a "Socialist" or a "Christian"; his influence on their ideas was surprisingly slight. The Methodist preacher Hughes is the only Nonconformist to appear in the book and echoes the theme of Headlam: "the distinction which is so commonly made between the secular and the sacred is a deadly anti-Christian delusion" (p. 161). Bishop Westcott gave respectability to Christian Socialism as president of the Christian Social Union in 1889; he kept it out of the hands of "Radicals and Liberals." During the Durham coalminers' strike of 1892, Westcott was like an Anglican Cardinal Manning, exuding social compassion. In a famous speech at the Hull Church Congress in 1890, he said wage labor would vanish in time, like slavery or serfdom, but he did not pursue the line of thought to its conclusion. He took the rhetoric of Christian Socialism and applied it to the program of social reform.

The Christian Socialists always thought of the capitalist system as being modest: moderate-to-small-sized firms. They never dreamed of oligopoly capitalism, which Victorian capitalism became. For Maurice, Henry Mayhew's picture of the small-trade world of the London costermonger

was "Capitalism." For Maurice and those who followed him all that was needed was adult education, a hint of trade unionism, and cooperative enterprise (or, after 1906, Guild Socialism).

Edward Norman's book has a parting shot: he suggests that Christian Socialism was symptomatic of autonomous social change, not an original cause of this change. But the Christian Socialists themselves discerned ultimate meanings and moral lessons in the conditions of their day for the working class and the whole of British society. They saw in "Christian Socialism" a radical departure from the conventional wisdom and the received attitude of their peers.

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ZUZANNA SHONFIELD. *The Precariously Privileged: A Professional Family in Victorian London*. New York: Oxford University Press. 1987. Pp. ix, 266. \$34.00.

Zuzanna Shonfield's account of an upper-middle-class family in Victorian London is based on diaries kept by Jeannette Marshall from 1870 to 1892 and on other family papers. Shonfield has written a history of three generations, from the marriage of Jeannette's parents in 1854 to Jeannette's death in 1935.

Jeannette's father, John Marshall, migrated to London to complete his medical training at University College. He rose to an eminent position in the profession, combining an increasingly lucrative general practice with distinguished posts in surgery at the University College Hospital and winning many honors. A renowned anatomist, he lectured on the subject at the Schools of Art and Design and the Royal Academy. His early patients included pre-Raphaelite artists, and he continued to associate with London's artistic and bohemian community. After marrying Ellen Williams, he rented a house in fashionable Savile Row, where four children were born.

The two daughters were educated at home by governesses. Their father, a supporter of women's higher education and activism, then insisted his daughters further their education with courses at University College. But Jeannette and Ada, backed by their mother, preferred the more conventional pursuits of walks, shopping, calls, dances, music, sewing, embroidery, and interior decorating. Jeannette's goal in life was to find a husband, but successive flirtations and courtships came to nothing. Only in her mid-thirties did she finally marry the family's new doctor. Apart from the birth of one daughter, her activities and interests remained much the same.

Shonfield has skillfully reconstructed from diverse sources the lives of the Marshalls and their London environment. She objectively analyzes each family member's personality and the inadequacies of their interrelationships, stressing an underlying theme of insecurity, not only financial but also social and psychological.

Yet, although Shonfield compares John Marshall's career to the London medical profession as a whole, she fails to relate Jeannette's life to the historiographic debate on upper-middle-class women. The few historians who have studied these women have generally accepted the Victorian ideal of the pious, poorly educated, domesticated, and delicate "angel in the house." But M. Jeanne Peterson, in her study of the Paget women (*AHR* 89, [1984]), challenged this stereotype as inadequate and inaccurate. Shonfield could have made a more significant contribution by comparing and contrasting Jeannette with both the stereotype and the Paget women, some of whom were wives and daughters of medical men. The Marshall ladies refused, in fact, to undertake many of the accepted roles of True Womanhood. Antisocial and querulous, Ellen was not energetic in advancing the family's social standing through entertaining. She and Jeannette showed little interest in their servants and their work. And none of the Marshall women were religious or willing to engage in philanthropy. John's daughters rejected the opportunities he offered, demonstrating that women themselves might choose not to broaden their lives. Lacking analysis within the wider context of women's history, this book's main value is its accessibility as a primary source for historians of medicine, London life, and middle-class women and families.

JESSICA GERARD
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HARRIET RITVO. *The Animal Estate: The English and Other Creatures in the Victorian Age*. Cambridge: Harvard University Press. 1987. Pp. viii, 347. \$25.00.

Harriet Ritvo's intriguing book is, as the subtitle implies, only tangentially about animals. Ritvo focuses, rather, on the rhetorical significance of human-animal interaction in the Victorian age. She begins by raising some theoretical questions about Victorian "discourses" on animals and illustrates these questions by a discussion of a number of popular nineteenth-century books, from Thomas Bewick's *General History of Quadrupeds* (1790) to Arabella Buckland's *The Winner in Life's Race; or, The Great Backboned Family* (1883), a children's book whose debt to Darwinian thought is clearly indicated in the title.

In one of the most interesting sections of the book, Ritvo shows us the ways in which the "scientific" classification of the animal kingdom was, in fact, influenced by a number of very human concerns. Thus, animals were often classified as much on the basis of the functions they served for people as on other taxonomic schemes. The dog, which "knew its place" and accepted the domination of humans was, therefore, considered a "nobler" creature than the cat, which refused to subordinate its will to that of its human masters.

The theoretical introduction is followed by three sections, each containing two independent but linked essays chosen to illuminate a particular theme. A chapter on the metaphoric content of aristocratic stockbreeding is linked with one on the more middle-class world of dog breeding and dog shows; a chapter on the movement against cruelty to animals is linked with one on the responses to rabies; a chapter on zoos and menageries is linked to one on big-game hunting.

Each of the separate studies contains a great deal of fascinating, if not always surprising, material. We learn, for example, of the way in which the "aristocratic" cattle exhibited at great agricultural shows throughout the century demonstrated not only their own superiority but also that of their aristocratic owners. By contrast, we are shown how the activity of dog breeding, an activity practiced mainly by the urban middle-class, challenged rather than affirmed the social hierarchy. The chapters on cruelty and rabies illuminate Victorian attitudes toward the poor and marginal members of society. The chapters on zoos and hunting show how animals could be used to symbolize and affirm the imperial enterprise.

Despite the strength of the material and the value of many of Ritvo's insights, I must add that my reaction while reading the book was, as often as not, a frustrated "yes, but . . ." Though in many ways a work of history, Ritvo's study is, in fact, a product of literary criticism: she is interested primarily in the rhetorical aspects of human-animal interaction. But past lives are not merely texts to be deconstructed; human activity is not mere "discourse." Failure to realize this leads to a very one-dimensional picture of a rich and complex reality. It also produces a tone of thinly disguised contempt on Ritvo's part for all of her human subjects. No human activity can be accepted for what it seems. Rather, it has to be seen only for its symbolic, rhetorical, or metonymic value. Thus, Ritvo cannot understand, for example, that Victorian ignorance about the causes of rabies might simply have been ignorance rather than the symptom of something more malign. In extreme cases she is led to some very curious positions. In the space of a few pages, for example, she condemns

public-health measures to contain rabies because of the rhetorical stance they display, but then she has to admit, almost in passing, that they did, in fact, succeed in controlling the disease.

Ritvo opens a number of fascinating windows on Victorian society, but the book is also as much the prisoner of its own rhetoric as it claims the Victorians were of theirs.

DAVID C. ITZKOWITZ
Macalester College

GRACE SEIBERLING. *Amateurs, Photography, and the Mid-Victorian Imagination*. Assisted by CAROLYN BLOORE. Chicago: The University of Chicago Press. 1986. Pp. x, 195. \$34.95.

"Who formed the first photographic society, and established the first photographic journal?" The answer to this question, posed in Britain in 1863, was simple: the amateurs. From clergymen to members of Parliament, from chemists to gentlemen of independent means, amateurs created the earliest achievements in Victorian photography. The nature of their technical and organizational activities is the basis of Grace Seiberling's study.

Tracing the emergence of photography from a curiosity to a mass medium, Seiberling provides a detailed account of photography's development from early failures and partial successes to a professional occupation and commercial enterprise gaining large appeal through such means as the *cartes de visite* and reproduced art works. Characterizing the ambivalent view of photography in the 1840s was its designation by the first organization of photographers, the "Photographic Club," as "*art-science* (we scarcely know the word fittest completely to designate it)" (p. 8). What Seiberling studies, however, is not the cultural but the social development of photography through the emergence of photographic exchange clubs, which formed to share information and images. Her main interest is the Photographic Exchange Club founded in 1854.

The subjects favored by the early amateur photographers were historic monuments, landscapes, still lifes, and reproductions of art, all shot in a picturesque manner to enhance the images. But, as Seiberling notes, many photographs of the 1850s lacked an emphasis on detail, partially due to the technical limitations of the negative; however, this lack of detail also reflected an aesthetic that gradually changed as the emphasis on clarity, technically possible in the 1860s, replaced a decision intentionally to blur detail for artistic effect, a position Sir William J. Newton advocated in the first address to the Photographic Society in April 1853.

In addition, as photography was able to capture objects in motion, the entire aesthetic of the picture shifted: exactness in everyday life and portraiture became the goal. Lady Eastlake in 1857 summarized the change: photography was no longer an experiment combining art and science but a controllable process to reproduce cheaply images for the masses (p. 43). The acceptance of collodion as a fixer for photographic emulsions and the substitution of albumen prints from salted paper paralleled the move to exactness and control of the image that made clarity of detail the fundamental feature of photography. In her study, Seiberling provides a carefully documented account of the developing images and processes that altered the character of photography in the mid-nineteenth century. Supplementing the text are seventy-eight black-and-white photographs recording iconographic changes, a biographical account of members of the Photographic Exchange Club, and a technical appendix.

IRA B. NADEL
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LOGIE BARROW. *Independent Spirits: Spiritualism and English Plebeians, 1850–1910*. (History Workshop Series.) New York: Methuen or Routledge and Kegan Paul, London. 1986. Pp. xii, 338. Cloth \$34.95, paper \$17.95.

In the last twenty years, historians have traced a plethora of Victorian “quack” movements that a previous generation deemed unworthy of attention. Mesmerism, phrenology, homeopathy, and similar movements have been lovingly restored to their place in the Victorian social landscape. Logie Barrow’s study is an important contribution to this effort. In contrast to previous historians of Victorian spiritualism, Barrow shuns elite organizations and concentrates instead on “plebeian (or working-class plus lower-middle-class) spiritualism” (p. 16).

Barrow sees plebeian spiritualism as the culmination, and last significant manifestation, of nineteenth-century “democratic epistemologies.” In the late twentieth century, persons without a doctorate in physics do not usually pretend to have definitive ideas about the nature of matter. This was not so in the nineteenth century. Some of the most basic questions about the nature of matter and health were contested in rough lecture halls and in popular periodicals. At one level there was a mentalist tradition, rooted from the 1820s in phrenology and later in mesmerism, that emphasized the individual’s ability to unlock capabilities of the mind that had been inadequately understood. At another level the barbarities of heroic

medical therapy spawned a series of noninterventionist medical systems, most notably homeopathy and hydropathy. At a third level the demise of working-class political movements by 1850 turned some Victorians of a reforming temperament toward health- and morality-oriented movements: temperance, vegetarianism, and opposition to compulsory vaccination and the Contagious Diseases Acts. Finally, older traditions of millenarianism and secularism found new forms and new advocates in the late nineteenth century. Common to all of these movements was a keen belief that truth could alter one’s life completely and that truth was accessible to every earnest seeker. Barrow examines each of these traditions and considers its relationship to spiritualism. Wisely, he eschews a simplistic genealogy and frankly admits that “we should talk less in terms of line of descent than of points of blur and tension” between spiritualism and other “democratic epistemologies” (p. 10).

Barrow examines these connections through minibiographies of a number of Victorians: J. G. H. Brown, the prophet of Nottingham, herbal medicine salesman, and spiritualist; James Burns, lecturer on phrenology and mesmerism, vegetarian, teetotaler, ascetic, and plebeian journalist of spiritualism; Spencer T. Hall, shoemaker’s son, phreno-mesmerist, homeopath, hydro-path, and spiritualist; and many others. Barrow’s presentation of the movements’ entanglements through the lives of their participants is one of the book’s real strengths. Another is his fascinating discussion of the relationship between spiritualism and Victorian death rituals (chap. 8).

The book’s only significant weakness is the author’s undisciplined and confusing prose style. Most annoying is Barrow’s prolific use of dashes, semicolons, colons, and parentheses to tie together thoughts that ought to be presented in separate sentences. This makes an otherwise stimulating book very difficult to read.

TERRY M. PARSSINEN
Temple University

PATRICIA E. MALCOLMSON. *English Laundresses: A Social History, 1850–1930*. (The Working Class in European History.) Urbana: University of Illinois Press. 1986. Pp. xv, 220. \$26.95.

In the 1969 reprint of her pioneering work, *Women Workers and the Industrial Revolution, 1750–1850*, Ivy Pinchbeck noted that, in the history of women, “the occupational changes which played so large a part in their emancipation” between 1850 and 1950 had “been curiously neglected.” Although Pinchbeck thought that many of the

technological changes occurring in that period had brought about new opportunities for non-manual employment that were fit subjects for study, it was also true that the more traditional fields of women's work needed analysis as well. Patricia E. Malcolmsen's book is an attempt to answer Pinchbeck's call through a study of "a substantially unskilled, ill-paid, poorly organized, highly seasonal, and often physically isolated occupation" (p. xi).

Because of the high preponderance of women in the laundry trade (for example, 55.9 percent of laundry workers were shown as either married or widowed in the 1901 census), Malcolmsen believes that laundresses provide "a case study through which a number of themes in economic, social, labor, and women's history can be explored." Although it may be true that such a study can enlighten us on a variety of social and political issues—such as "the relationship between home and workplace" (p. xii), changes in industrial processes, consumerism and working conditions, state intervention into conditions of labor, and feminism and women's work—the attempt to do so in one volume does not allow for an in-depth treatment of many of the issues.

The book begins with a survey of laundry work, including a discussion of how clothes were washed, how laundresses were viewed, and where, geographically, laundry workers were found. Malcolmsen's next chapter focuses on "Hand Laundry Work and the Family Economy," which includes such topics as "The Hand-Laundering Process" and "Laundresses as Parents." From here, she moves on to the debate over protective legislation, the effects of regulation, and the attempts to unionize laundry workers. Her final chapter deals with mechanization and social change.

Malcolmsen does make several important points that help our understanding of women's work in manual occupations. For instance, she emphasizes the casual nature of the trade in that married women often became laundresses when their husbands were out of work or were ill or incapacitated. It was most like a safety net for these women and their families. Malcolmsen also claims a certain empowerment within the relationship between husband and wife for those laundresses whose employment "was an important part of a dovetailed family economy," a point that deserves more than a passing comment in the study (p. 43). As she does emphasize, however, the very advantages were "obstacles" to improvement; they made laundresses difficult to organize into unions and kept wages low (p. 118), at least before Trade Board regulations were applied.

Malcolmsen also delineates the various feminist positions on protective legislation in the last quar-

ter of the nineteenth century as an example of how the study of one trade can help us understand the formulation of social policy during a specific time period (p. 46). She does not, however, articulate sufficiently the growing social feminist stance that stressed both protection of women workers and fairness toward them. This position was epitomized in the work of Clementina Black and her colleagues, first through the Women's Trade Union Association and then through the Women's Industrial Council.

Malcolmsen's research has uncovered many interesting facts about the life and work of English laundresses, but unfortunately she does not heed her own judgment about the difficulties of making generalizations about the laundry industry (p. 136). Perhaps because of the amount of information she has to convey, one wonders if indeed it would not have been better for her to have written two books: one on the life and work of women in the laundry trade and the other on the structure and mechanization of the industry over the period under study. Certainly the life and work of women as laundresses is the most interesting part of her book and the most important for historians of women.

ELLEN F. MAPPEN
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MICHAEL SAVAGE. *The Dynamics of Working-Class Politics: The Labour Movement in Preston, 1880–1940*. New York: Cambridge University Press. 1987. Pp. xii, 280. \$39.50.

How labor politics are produced is the fundamental question for labor history to answer. The tradition established by Beatrice and Sidney Webb—and still very much alive in its original or revisionist forms—answered that question in terms of the growing rationality of organized interest group bargaining and competition. In the late nineteenth century, it made sense to believe that this meant statist politics informed by socialist ideology. Such a connection now seems less natural and more problematic. But those historians who have quite properly questioned the teleology that equated labor with socialism have too often engaged solely in intellectual nihilism and failed to provide an adequate alternative formulation of the production of politics. The prime virtue of Michael Savage's book is that he makes this attempt.

In this study of popular politics in Preston, Savage provides an intelligent discussion of the relationship between working-class consciousness and labor politics, explaining how both grow from the dependency and insecurity that is integral to

capitalist social relations. This dependency produces an intensely practical politics whose strategies do not imply a preordained ideological stance, nor do they proceed in linear fashion, but vary over time and are conditioned by the nature of social relations.

The successful appeal of political parties to the labor vote rests on the extent to which they can mobilize the existing "capacities" for working-class collective action at the local level. These capacities are produced by the particular structures of social relations, especially, in this account, by the structure of the labor market and gender relations. Thus, where Labour Party politics reflected solely the mutualist concerns of skilled craft workers and failed to include women or unskilled workers (as was the case in Preston and generally in the late nineteenth century), its appeal and success would be severely limited. Preston was noted for the success of popular Conservatism, which Savage shows rested on the ability of an astute Tory leadership to appeal to the economic interests of the skilled male textile workers and on the Conservative hold on structures of neighborhood collectivities such as the churches.

Changes within labor politics—both in terms of overall political attachment and internal policy prescriptions—result from changes in social relations. Thus the threat to male control of the textile labor market from female workers in the early twentieth century placed local labor politics in opposition to women's issues. By the 1920s, however, the overlooker's traditional patriarchal control of the textile labor market had been undermined by employer strategies and state policies. Partly as a consequence, but also because of the war, neighborhood collectivities had emerged that represented wider working class interests, diluted trade union dominance of labor politics, and allowed women's and consumer issues to enter the political discourse. Problems such as education, child support, and social services began to be pressed by the local party in contrast to the previous exclusive emphasis on wages. By the 1930s, mass unemployment had undermined the structures on which such an expression of labor politics rested and, incidentally, allowed the reassertion of a more purely trade union control of the party.

Savage has made an intelligent and worthy effort to reformulate the traditional (and now much-despised) focus on the relation between social structures and politics. Indeed, he successfully reestablishes the fundamental importance of that paradigm. But he also reminds us of two very important points that have often been ignored in the discussion of the growth of independent labor politics. First, it is to the *local* rather than the national picture that we must look to understand

working-class political mobilization. Second, even if social relations remain the key to understanding those politics, they must be extended beyond production to include gender relations and networks of neighborhood community. Indeed, Savage makes a sensible case for the argument that it is here that the explanatory dynamic of labor politics may be found.

RICHARD PRICE
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E. P. HENNOCK. *British Social Reform and German Precedents: The Case of Social Insurance, 1880–1914*. New York: Clarendon Press of Oxford University Press. 1987. Pp. vi, 243. \$49.95.

E. P. Hennock has written a study of the policy and implementation of British social insurance in the three decades before 1914. His principal focus is on the Bismarckian precedent of the 1880s and on what persuaded leaders of both the Conservative and Liberal parties, as well as those of the trade unions and the new Labour party, at first to reject and then to accept the principles and methods of the German system. Hennock sets his study amid the efforts of a number of writers of the time who urged that England turn from the amateurism and muddle of its institutions to German professionalism and efficiency, warning that otherwise Germany would pass England by.

There were three principal phases. The first led to the passage in 1897 of an act holding employers liable for compensation for industrial injury, regardless of fault. Joseph Chamberlain, the former Radical and, at the time, Tory colonial secretary, was the leading figure in this effort. This act had no provision for compulsory insurance as did the German law, thus leaving out of luck a worker employed by a bankrupt company. Compulsion, and with it state control and bureaucracy, was a German not a British policy, both parties agreed, and must be avoided. Even the trade unions accepted this argument, conscious that Bismarck intended his system to dish the socialists. The second phase concluded with the Liberal government's old-age pension legislation in 1908, a non-contributory scheme financed out of general taxation that, given budgetary difficulties, meant inadequate pensions at the advanced age of seventy. Again the German idea of compulsory insurance was ruled out. The third phase saw the conversion of David Lloyd George and Winston Churchill, the leading social reformers in the Liberal cabinet, to the German model of a compulsory, contributory system, preparing the way for the national insurance legislation of 1911 based on German principles. Before accomplish-

ing this, Lloyd George and Churchill had to persuade the trade unions and the leaders of the mutual-help friendly societies that such a scheme would not undermine their traditional roles or thwart the growth of the new Labour party.

Although it is probably useful to absolve, as Hennock does, the Liberal social reformers of the Bismarckian machinations that may have moved a Tory Chamberlain, it is not clear why the author fails to take into account the working-class agitation and strikes during the prewar years and the Labour party's threatening proclamations of proletarian solidarity against international capitalism. Both major parties worried about working-class loyalty and wished to give the working class a firmer stake in the nation. What may have led the author to ignore this question may have been his decision to deal only with the free trade, not the protectionist, exponents of the German model, though we are not told why. Chamberlain, long an admirer of Bismarckian social legislation, campaigned for old-age pensions financed out of tariff revenues in the Tory protectionist program of 1903. Similarly slighted by Hennock were the huge amounts of money required to maintain the Royal Navy's superiority over a growing German fleet; this helped create the budget stringencies that made it necessary to turn to a compulsory contributory scheme if national insurance were to be both universal and financially feasible.

Hennock has nonetheless provided scholars with a most useful study of how an insular England turned to the example of a foreign rival—with a very different political culture—to solve serious social, political, and financial problems. His comparisons of the British and German systems of national insurance are intelligently presented, and his work will prove most valuable to students of British social policy.

BERNARD SEMMEL
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BRIAN W. BLOUET. *Halford Mackinder: A Biography*. College Station, Texas: Texas A&M University Press. 1987. Pp. ix, 235. \$21.50.

If Halford Mackinder is remembered today, it is normally for some phrases such as "the Heartland" or "the World-Island," which illuminate his geographical theory that world dominance would come fairly easily to any political entity that united Germany and Eastern Europe and Russia. The theory was outlined in 1904 when German *welt-politik* was beginning to attract attention, was fully stated in *Democratic Ideals and Reality* (1919) just after the First World War indicated the possibility

of German dominance of the Heartland, and enjoyed a revival in the 1940s when first Germany and then Russia looked like they might reach positions of dominance. Perhaps we would now pay more attention to demographic facts, but Mackinder wrote at a time when India was ruled by an imperial power and when China was showing symptoms of the problems that had made imperial power over India so easy to win and to maintain.

A theory that helps explain an important aspect of the struggles of the last hundred years deserves attention. Does the author deserve a biography? Clearly Mackinder did a number of interesting things, but he did not have those quirks of personality, or those crowds of friends, that make biography the easiest form of history to popularize. In his twenties he realized, certainly without any example in Britain to follow and apparently without any influence from overseas, that geography was a serious intellectual discipline, and he devoted himself to gaining a stronger position for it in the British educational system. When he found that one powerful group in the Royal Geographical Society was uneasy about his support for the subject because he had done no proper geography (by which the group meant exploring), he organized a successful expedition up the previously unclimbed Mount Kenya and then showed no further interest in exploring and not much interest in climbing mountains. Because he combined enthusiasm for geography with dedication to university extension work, he found himself commuting from Oxford to Reading to lay the foundations of a new university there. When his new Geography School at Oxford was sharply divided by internal disputes, he departed to become director of the London School of Economics, was tempted from this post by financial support from Lord Milner to become a propagandist for imperial preference, and conferred some intellectual distinction on the House of Commons during a dozen years as a Member of Parliament. Afterward he settled down to twenty-five years of useful work in the civil service as chairman of the Imperial Shipping Conference. In the 1940s, as the struggle for the Heartland reawakened, attention was paid to him again, but he did not add much to an idea that had already shown its potency.

So, Mackinder was a man who was worth a biography, and it is reasonable to ask if he could have had a better one. Brian W. Blouet's style is curiously and perhaps appropriately flat; he uses simple sentences with hardly an example of complexity of expression to suggest complexity of thought. Beatrice Webb suggested that Mackinder was a man without distinction, and perhaps the

style of this book lends support to her view. The author, a geographer, knows far more about political history than most historians know about geography, but, at a few points (such as his speculation that Mackinder might have got into the cabinet in 1922), he is a little out of touch. On the other hand, his approach sometimes reminds one to take a fresh look at things. Blouet devotes more space, for example, to the founding of Reading University than to the whole of Mackinder's political career, and on reflection this is probably justified. Mackinder left some solid things to mark his life, and Reading was one of them. He may have been lucky to be around when geography as a subject and university extension as a form of organization were coming forward, but other people had his opportunities and made much less of them. To understand the world one has to see how the system works, and Mackinder emerges from this biography as the sort of man who made it work.

TREVOR LLOYD
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KEITH GRIEVES. *The Politics of Manpower, 1914–18*. (War, Armed Forces, and Society.) New York: St. Martin's. 1988. Pp. vii, 241. \$45.00.

Keith Grievess's book will inevitably invite comparison with another recent study of the manpower question, *The Conscription Controversy in Great Britain, 1900–1918* (1987) by R. J. Q. Adams and Philip Poirier. Both books do, in fact, cover some of the same ground, namely, the importance of the manpower problem as a general theme in modern British society as well as a specific aspect of wartime political struggles, especially the crisis of December 1916 that ended with David Lloyd George as prime minister. Grievess's work, although narrower in scope, has more depth since it considers every facet of the manpower question while the Adams and Poirier volume deals mainly with conscription. But Adams and Poirier's traditional political approach provides a richer, more elaborate texture and, frankly, makes it a "better read." This is not to disparage Grievess's exhaustively researched, well-conceived, and thoroughly competent monograph but only to point up that, like most histories of modern bureaucracy, this book is not likely to elicit great waves of excitement. The prose is dense and unrelenting; the author says what he wishes to say with very little "wastage" (to borrow a terrible euphemism of Great War vintage). Economy is the watchword with regard to both the style of the text and the infusion of humor into it (Douglas Haig and Neville Chamberlain probably never did laugh,

but surely Christopher Addison, Sir Auckland Geddes, or Lord Derby chuckled now and again). Still, Grievess's intention is obviously not to entertain but to inform, and he delivers a wealth of useful information in his "consideration of the manpower problem as a crucial dimension of civil-military relations in Britain" (p. 3).

The roots of Britain's manpower crisis lay in a general failure to recognize that winning a war between powerful industrial nations requires more than simply putting armies into the field. Hence, during the first year of the war, the haphazard raising of Earl Kitchener's New Armies without central control or differentiation between the skills of civilian recruits exacerbated an inherent problem that "would grow incessantly" (p. 17) throughout the war. The imposition of conscription in early 1916 was of little immediate help because it merely created a situation in which various departments—the War Office, Munitions, Agriculture—vied with one another to draw men from a diminished and diminishing pool. Thus, while generals in France complained about both the quantity and quality of war materials they received, the War Office regularly attacked the Ministry of Munitions for "stealing" men that the army should have had.

One of the most compelling points to which Grievess consistently returns is the persistent War Office/General Staff view that the size of the army in France "was the only claim on the nation's supply of manpower which was worthy of consideration" (p. 203). When General Sir William Robertson told Lord Midleton in September 1916 that every man needed to be "put in his proper place" (p. 44), he really meant that every man the army wanted for bloody disasters like Loos, the Somme, or Passchendaele should be put into uniform and shipped to France. The generals altered their profligate use of manpower, Grievess says, only after the War Cabinet in early 1918, under the advice of Sir Auckland Geddes's Ministry of National Service, fully asserted its responsibility for manpower distribution by establishing shipbuilding and airplane and tank construction as higher priorities than replacing "wastage" on the Western Front.

In tracing various attempts to come to grips with the manpower crisis, from the introduction of the National Register in July 1915 to the establishment of the Ministry of National Service in September 1917, Grievess disputes the view that the organization of British manpower was drastically altered by Lloyd George and his "men of push and go." During his first year as prime minister, Lloyd George's responses to the manpower problem were largely drawn from Asquithian precedents and achieved the same inadequate results. Grievess

believes, for example, that Lloyd George was much to blame for Chamberlain's failure as director of National Service because he neglected to provide Chamberlain with either a clear conception of his role or with meaningful support for the voluntary National Service plan that Chamberlain attempted to implement. Not until Chamberlain's successor, Sir Auckland Geddes, demanded and received ministerial power and open lines of communication with the War Cabinet did the British government have the "capacity for the comprehensive reform of . . . manpower" (p. 152). The lesson "that the control of military manpower was too important . . . to be left to the generals" (p. 210), if only belatedly recognized in 1918, had at least, Grieves concludes, a salutary effect on the organization and use of manpower by Churchill's government in World War II.

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JOHN W. GORDON. *The Other Desert War: British Special Forces in North Africa, 1940-43*. Foreword by THEODORE ROPP. (Contributions in Military Studies, number 56.) Westport, Conn.: Greenwood, 1987. Pp. xxiii, 241. \$39.95.

Special military organizations survive not because of their utility but because of their sponsorship. Often emerging from the persistence of zealots rather than from a need recognized by conventional forces, these units fight two battles. The external struggle focuses on the enemy while the internal, and usually more deadly, conflict rages against the bureaucracy that reluctantly parents the offspring.

John W. Gordon captures both the essence and the nuance of this dual combat. His description of British special units during the North African campaign identifies the origins of a debate still unsettled today. World War I experience and interwar expeditions gave the British an initial advantage that became critical when they adopted a posture of economy of force. Exploiting this advantage required the formation of special forces. These forces were special because they used techniques and equipment particularly suited to the environment and because they undertook missions deemed too hazardous for conventional troops.

The Long Range Desert Group (LRDG) was the first of these tailored forces and began as a deep reconnaissance unit. Success led to its expansion and to the formation of other units. The Special Air Service (SAS) and Popski's Private Army (PPA) were the most notable. The SAS concen-

trated upon direct-action missions against enemy convoys and airfields, while the PPA dressed and equipped German-speaking soldiers to infiltrate and destroy targets. When participating in the small-unit actions for which they were designed, these forces achieved spectacular results. When involved in activities on a larger scale, they usually failed miserably. Gordon clearly shows that bringing diverse, special units together does not necessarily produce a symbiotic relationship. Throughout their battles, the special forces experienced varying degrees of support and control from higher headquarters. Constant turmoil arose over use of these troops, whether they would be strategic assets or adjuncts to the tactical force. Invariably, control by tactical commanders led to disaster, and transfer of these environmentally tailored assets to other theaters spelled demise for all save the SAS.

Anchored firmly on organizational records and individual papers, Gordon's first-rate scholarship details an exciting story of this "other war." It moves beyond earlier works of individual groups and actions to weave the stories of disparate units into a tapestry of continuity and completeness. Gordon accurately describes the operation of special units in the past and, uncannily, the present. The lessons of this unique, outstanding work, if unheeded, may also predict the future of such organizations.

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MESBAHUDDIN AHMED. *The British Labour Party and the Indian Independence Movement, 1917-1939*. New York: Envoy, 1987. Pp. xii, 215.

This is a welcome addition to the existing literature on Indo-British relations in the pre-independence era. It is a good piece of research and covers an important period in the annals of modern India. It is appropriate that Mesbahuddin Ahmed has chosen to commence his investigation from 1917, the year in which the epoch-making declaration of E. S. Montagu was made, and to conclude in the year 1939, which saw the outbreak of World War II. In between these two key dates, a host of remarkable developments relating to the Indian national movement took place.

It is but natural that Ahmed has chosen these developments to examine the British Labour party's attitude toward the Indian independence movement and find out how the leaders of the Labour party thought and acted during the most crucial phase in the annals of Indo-British relations.

So far as colonial problems were concerned, there was no difference in the attitudes of the British political parties, whether Conservative or Labour. They did not have definite policies, and decisions—mostly vague—were made according to expediency. In the 1930s, no British party was prepared to concede independence or even full dominion status to India.

There is no gainsaying the fact that the Labour party, unlike other political parties in Great Britain, was friendly to India's nationalistic aspirations. But that does not mean that it wholly supported all that the Congress party did during this period. Ahmed has done well to examine the origin of the British Labour party. He has endeavored to rely on available primary sources and secondary sources. Although the sources to which he had access were somewhat meager, they were adequate to enable him to develop his thesis.

Ahmed's findings cannot be brushed aside as irrelevant or insignificant. He rightly says that the British Labour party's Indian policy began only in 1918, although the party came into being much earlier. His assessment of the first minority Labour government's attitude vis-à-vis the Indian national movement seems very realistic. His assessment of the manner in which the second Labour government handled the Indian question seems very unrealistic and optimistic. It is difficult to concur with Ahmed's contention that it would not have been a formidable task for the Labour government to muster adequate support to push through the British Parliament legislation to fulfill the Labour party's promises to India.

There are two very serious lacunae in the work. The adoption by the Congress party at its Nagpur session in December 1920 of the resolution on the attainment of Home Rule (swaraj) by all peaceful and legitimate means and, at its Lahore session in December 1929, the resolution on the attainment of complete independence as its ultimate goal constitute significant developments in the evolution of the political ideology of the Congress party that tremendously influenced the nature and course of the Indian national movement. But Ahmed has not examined in any detail the Labour party's attitude toward these ideological developments in the Congress party. There is another defect in this work; Ahmed has done precious little to examine the British Labour party's attitude toward the efforts made by the All-India Muslim League to counteract the Indian national movement led by the Congress party.

Notwithstanding these shortcomings, Ahmed has ventured to contribute an interesting addition

to the existing field of literature on the Indo-British relations of the pre-independence era.

K. VEERATHAPPA
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EMMET LARKIN. *The Consolidation of the Roman Catholic Church in Ireland, 1860–1870*. Chapel Hill: University of North Carolina Press. 1987. Pp. xxi, 714. \$49.95.

For nearly three decades, scholars in Irish studies have been watching the work of Emmet Larkin on the Roman Catholic Church in nineteenth-century Ireland with a mixture of awe and anxiety. The awe reflects the nature of the task he has undertaken, the anxiety the possibility that he may never complete it. This volume is an impressive achievement in itself and is simultaneously a token that, yes, Larkin will complete the task that he has set himself, which is to tell the full story of the single most important institution in nineteenth-century Irish history. Readers who pick up the present volume should understand that it is not a thing in itself but a fifth installment of a project that eventually will cover ten or eleven volumes, depending on how the publisher wishes to package it.

The most salient thing to know before entering Larkin's scholarly world, however, is that it is virtually a hermetic kingdom. One will find no trace of the methods of modern religious scholarship, no indication that Larkin has read any works by social scientists or theories of organizational behavior (even by Max Weber), and will discover little awareness of work in Irish studies outside the immediate time frame in which he operates. That a book of over seven hundred pages has only a two-page bibliographical note is an accurate reflection of this situation. It matters not. Larkin's series of books has a fierce and admirable integrity of its own and is so rich in detail, so full of ecclesiastical arcana, that one can enter its world the same way one enters the world of, say, C. S. Lewis. Thus, new readers should not be put off by Larkin's scholarly eccentricities. For example, he has invented his own system of footnotes. It works, and, once one has become accustomed to it, one finds it no more distracting than any other system of documentation.

One reason that this series will long remain valuable is that, for the most part, Larkin eschews analysis and instead presents great blocks of quotations from letters in ecclesiastical archives. This is done on a scale so lavish as to have been rarely seen in the scholarship of the British Isles since the Edwardian era. As a result, later generations of scholars will be able to mine Larkin's work as virtually a primary source.

If the Catholic church in Ireland saw the world as a spiritual battlefield, then this is a study of spiritual generals. Subalterns (the parish priests) are largely absent as distinct individuals, and the foot soldiers (the Catholic laity) are hardly mentioned. But the generals—the bishops and especially the field marshal, Archbishop Paul Cullen—are in the spotlight. The 1860s were a hard decade for the bishops, faced with the rise of Fenianism and widespread economic distress, but they emerged victorious by 1870. The Protestant Episcopal church was disestablished in Ireland, and the bishops consolidated their control over the national school system. Larkin's phrase for this victory was that the bishops had finally institutionalized the "corporate wholeness" of the modern Irish Catholic church (p. 686). There is a certain covert attitude of triumph in this viewpoint, but that is understandable. Most biographers, even institutional ones, are pleased when their subjects turn out to be winners.

This book is excellently produced by the University of North Carolina Press. In taking up the continuing publication of Larkin's monumental series, the editors in Chapel Hill are setting an excellent example of what a scholarly press should do.

DONALD HARMAN AKENSON
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EUNAN O'HALPIN. *The Decline of the Union: British Government in Ireland, 1892–1920*. New York: Syracuse University Press, 1987. Pp. xi, 258.

"Anomalous in quiet times, and almost unworkable in times of crisis" was how the Hardinge Commission, the royal commission appointed to look into the causes of the Easter Rising in 1916, described the Irish system of government. This was hardly an exaggeration. Four years later Warren Fisher, the permanent secretary to the Treasury, was sent to Dublin to investigate the workings of the Irish administration, and he concluded that "the Castle administration does not administer" (p. 207). The thorough reorganization that followed his report came too late to save the union, but it constituted a major and lasting legacy to the new Irish state that emerged in 1922.

The decline and fall of British government in Ireland has been a popular theme for historians. During the 1960s the work of L. P. Curtis on Arthur Balfour's attempt at "constructive unionism," Leon O'Broin's analysis of the failure of Augustine Birrell and the background to the Easter Rising, and R. B. McDowell's study of the Irish administration dominated the field. More recently

other scholars—most notably Charles Townshend, David Fitzpatrick, and John McCollan—have described some of the administrative problems leading to the breakdown of British rule in Ireland. Drawing on government records and the diaries and papers of leading British and Irish politicians and civil servants, Eunan O'Halpin both provides a useful synthesis of this earlier work and throws new light on the discussion. Beginning where Curtis left off with the constructive unionism of the Balfour brothers during the 1890s, through the devolution ideas that surfaced while George Wyndham was chief secretary, to the final collapse under Lord French, O'Halpin deftly and dispassionately maps the numerous land mines and pitfalls that awaited those whose frustrating task it was to administer Ireland during these years. In so doing he shows with great clarity why the landscape was so liberally strewn with political corpses—Wyndham's and Birrell's being the best known but by no means the only ones.

The constitutional issue overshadowed every aspect of administration in Ireland, from the promotion of development to the enforcement of the law. Neither the Nationalists nor the Unionists wanted fundamental reform, the Nationalists because they feared it might weaken the demand for self-government and the Unionists because they feared it might lead to self-government. The British Treasury jealously guarded the purse strings of the Irish administration and, once it became convinced of the inevitability of Home Rule, opposed all proposals to improve matters in Ireland either by increased spending or by giving Dublin Castle some financial discretion. And the Treasury took no constructive interest in the condition of Irish administration until the end.

By focusing almost exclusively on administrative issues, O'Halpin is able to bring into stark relief one of the main reasons for the British failure in Ireland. He is especially good on the catastrophic failure of British intelligence, in regard to both the Castle's failure to penetrate the Irish Volunteer and Irish Republican Brotherhood (IRB) organizations before the Easter Rising (although it is only fair to say that even Eoin MacNeill, the chief of staff of the Irish Volunteers, was deceived about the plans) and the related bizarre behavior of Admiral "Blinker" Hall in withholding vital information concerning German aid to Irish separatists and the timing of the rising from the Castle. And Hall did not hesitate to provide Dublin Castle with misleading information when it suited his purposes.

O'Halpin is a reliable guide, and the few errors (such as the incorrect date on p. 118 of the first battle of the Somme) are unimportant. His descriptions of the actors throughout are skillfully

and convincingly drawn. Perhaps inevitably, however, his conceptual approach has drawbacks. He is right to emphasize the usual ignorance and disinterest of the cabinet on Irish affairs, but this focus makes the study at times somewhat one-dimensional, especially since major policy making emanated from London throughout the period. O'Halpin's study is, however, a valuable book and an important contribution to Irish administrative history, although perhaps one that only readers already well versed in the intricacies of British and Irish politics during this period will be able to appreciate fully.

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RONNIE MUNCK and BILL ROLSTON. *Belfast in the Thirties: An Oral History*. New York: St. Martin's. 1987. Pp. 209. \$29.95.

Ronnie Munck and Bill Rolston are Marxist sociologists who live in Northern Ireland. Fifty people were interviewed for this project, most of them by Gerry Moore, their research assistant. Part 1, written by Rolston, deftly merges documentary analysis with oral material to yield accounts of the Outdoor Relief (ODR) strike by unemployed workers in 1932, the 1935 sectarian riots, and the experience of unemployment and poverty. The most significant contribution is the treatment of the ODR strike, which successfully counteracts the mythological view of it as an example of prerevolutionary class consciousness. The limitations of the involvement and goals of the participants are relentlessly but sympathetically demonstrated. We can accept the cognate argument that socialist and sectarian elements should be seen as coexisting rather than as mutually exclusive, so long as we acknowledge that sectarian elements overwhelmingly dominate. Part 2, written by Munck, approximates the form of a continuous stream of oral quotations, interspersed with orienting comments that generally lack the analytical sophistication of part 1. The material nevertheless documents the repeated splits in the Labour, Socialist, and Communist parties on the national question, demonstrates further symptoms of the overall political weakness of class, and provides rare glimpses of the organization of the Irish Republican Army in the 1920s and 1930s. Here and throughout the problems of verifying the actors' oral accounts of their political failures are acknowledged but not resolved.

The book fails to reach its declared goal of delineating the relationship between socialism and sectarianism because of a flawed treatment of sectarianism. Here Munck and Rolston extend the

long tradition of Marxist treatments of Northern Ireland, treatments that are crippled by greater sympathy for either Green or Orange forces. The problem is that, although the authors acknowledge that sectarianism can not be attributed only to capitalist conspiracy, they treat it as primarily a Protestant characteristic. They should reflect more deeply on the experience of the Protestant socialists whom Paeder O'Donnell persuaded to join a banned commemoration of the Easter Rising, a rare and signal achievement and just one of the arresting illustrations with which the book is replete. When the authorities blocked the parade, "the whole republican procession flopped down on its knees and began the rosary" (p. 183). This event illustrates, in this case on the Catholic side, the profoundly reciprocal quality of sectarianism in Northern Ireland. The actors are hypersensitive to the prejudices of the other side but do not realize that their own routine activities, so unproblematic and natural to them, are also deeply sectarian, or, if they do realize this, they are unwilling or unable to transcend them. This is a profound obstacle to socialism or any political solidarity among Protestants and Catholics. In failing to do justice to it, Munck and Rolston fall far short of the critical oral history that elucidates "the system of social/psychological/subconscious relationships which make sense of our experience," an elucidation they call for in an afterword.

This work is useful for its substantive material but severely compromised by its treatment of sectarianism. There is no index.

JOHN L. P. THOMPSON
Columbia University

CALLUM G. BROWN. *The Social History of Religion in Scotland since 1730*. (Christianity and Society in the Modern World.) New York: Methuen. 1987. Pp. xii, 273. \$63.00.

Scotland's tumultuous religious history has had a profound effect both in shaping the Scottish national identity and in leaving a legacy of bitter sectarian divisiveness. In this ambitious study Callum G. Brown surveys the troubled social history of religion in modern Scotland, giving particular attention to the impact of industrialization and urbanization on religious organization. Although sensitive to what is unique in Scottish religious history, Brown also maintains that modern Scottish developments must be viewed as parts of a larger British, or even Western, experience.

Brown's survey begins with the disintegration of the ideal of Scotland as a Calvinist commonwealth centered on a single national church. Between 1730 and 1850 the established Church of Scotland

experienced a series of secessions, leading to the formation of several competing Presbyterian denominations. During this same period, a number of non-Presbyterian denominations emerged to social significance, and the Roman Catholic church in Scotland revived. The main cause of this new religious pluralism, Brown argues, was the rise of commercial and industrial society, which generated new social antagonisms, new occupational groups, and new patterns of residential segregation based on social class and occupation. Emergent social groups in town and country defined their identities and sense of self-worth by forming themselves into sect-type denominations. They competed vigorously with other denominations—in an evangelical war of all against all—for converts or objects of Christian charity. The puritanical discipline and sense of mission maintained within the competing denominations helped their members cope with the rapid social change, profound deprivation, and glaring inequalities of early industrial society.

The spirit of puritanical evangelicalism began to wane, Brown observes, as the Scottish people gradually adapted to the urban-industrial landscape, and as a collectivist social welfare state developed to reduce social deprivation and inequality. As social anxieties and alienation decreased, sectarian fervor cooled and church attendance declined. The “crisis for religion” in Scotland, Brown believes, occurred between 1890 and 1929. These years, he notes, also witnessed a series of reunions among the main Presbyterian denominations in Scotland, as church leaders responded to diminishing attendance and collections by consolidating and rationalizing their organizations. Despite high hopes, however, the reunions did not halt the steady erosion of religious commitment, which Brown believes has now relegated religion to “the margins of social significance” (p. 256).

This is an important book that reflects recent research on such themes as working-class church attendance and the geography of denominational affiliation and that, drawing on the work of social historians in England and North America, presents a fresh view of religious pluralism in industrial Scotland. It is, to be sure, a contribution more to social history than to religious history; although Brown carefully analyzes the social determinants of religious organization and activity, he gives relatively little attention to the internal dynamic of Scottish religion, as manifested in popular beliefs, types of piety, or varieties of worship. Nonetheless, his book is a welcome corrective to denominational histories that often neglect social deter-

minants in their emphasis on doctrinal disputes and ecclesiastical structures.

STEWART J. BROWN
University of Edinburgh

FREDERIC J. BAUMGARTNER. *Henry II: King of France, 1547–1559*. Durham, N.C.: Duke University Press. 1988. Pp. xiv, 358. \$37.50.

The twelve-year reign over France of Henry II has often been seen as a critical episode in a momentous transition—from medieval to modern social organization, from a decentralized, multinational polity to a centralizing national state. Yet there has been no English-language biography of Henry II of France since Hugh Noël Williams's in 1910, and that work, which focused on the personal life of the king in the court milieu, did not supply the full-scale, scholarly political biography that Frederic J. Baumgartner now offers.

Indeed, Henry II also has been neglected by French sixteenth-century scholars, who have usually been more intrigued by the personalities of Henry's widow, Catherine de' Medici, or of his father, Francis I. Ironically, the first important French history of Henry II's reign since Henri Lemonnier's, in 1904, appeared only in 1985. Ivan Cloulas's massive *Henri II*, however, lacks the footnotes that make it possible to follow the leads by which knowledge is advanced. Baumgartner, on the other hand, has produced a coherent, readable, and scrupulously annotated overview of Henry's mid-century reign that seeks to expose the political role of the king himself.

Assessment of what part Henry actually played in the decisions of the regime has been impeded by the powerful or alluring personalities of Constable Anne de Montmorency, of the Guises, particularly the Cardinal of Lorraine, and of Diane de Poitiers. French historians, Cloulas included, have generally been inclined to believe that Henry was a passive instrument of these advisers and favorites and to see the policies of his monarchy as determined by shifting coalitions among them. By contrast, Baumgartner's provocative analysis gives Henry the benefit of doubts about his leadership. After systematically weighing the evidence and specialized literature on each issue, Baumgartner tends to dismiss Diane's part in resolving most political questions, to minimize the importance of Montmorency's advice, to stress the ambiguity in Lorraine's role, and to assign to the king himself the role of mainspring, as much in the religious and administrative policies of the regime as in the recapture of Calais and in the moves that ended in the peace of Cateau-Cambrésis.

Two chapters devoted to the regime's innova-

tive methods of warfare and finance, respectively, could serve as a lucid introduction to these aspects of sixteenth-century cultural transformation. A chapter on life at court describes Henry's limited role as a patron of the artistic and literary activity surrounding him.

Considerable space is given to matters of religion by Baumgartner, who is the author of two important monographs on sixteenth-century French religious politics (1975 and 1986). His interpretation of the king's apparently inconsistent religious policies is interesting: "Lutheranism" was seen by Henry as a danger to the state when it proved contagious among the common people in France. He did not fear Protestantism as a revolutionary belief system of the well-born either at home or abroad.

Baumgartner shrewdly examines Henry's emotional life for its political significance. The king had been a captive of Spain from the age of six to eleven. To Henry's suffering and resentment during this ordeal Baumgartner partly attributes his deep "hatred and fear of Charles V" and his "concentration on the dominant goal of his reign"—the emperor's ruin (pp. 166, 160). Baumgartner sees the king as "deeply interested in the son of his enemy" (p. 248), forming sanguine expectations of Philip II that may have been based on feelings dating to Henry's own youthful experience.

Except for the manuscript resources of the Bibliothèque Nationale, Baumgartner relies primarily on published collections. He does not claim to have plumbed all the sources listed in Cloulas's impressive bibliography. What he has presented, however, is a valuable, crisply written, meticulously documented history in which Henry II's role is judiciously and authoritatively appraised as the events of his reign unfold.

ELIZABETH WIRTH MARVICK
Los Angeles, California

ROBERT M. KINGDON. *Myths about the St. Bartholomew's Day Massacres, 1572–1576*. Cambridge: Harvard University Press. 1988. Pp. vi, 269. \$30.00.

This book is a masterful examination of the use of propaganda in the French religious wars. Robert M. Kingdon takes as his point of departure the pamphlets and political treatises written in reaction to the massacre of St. Bartholomew's Day and published as the *Mémoires de l'état de France sous Charles neufiesme* by the Genevan pastor Simon Goulart. Through a series of extended commentaries on these works, Kingdon seeks to illuminate the polemical intentions of their authors, to establish the circumstances under which they were

written, and, whenever possible, to explore their effective consequences. He examines the intended audience for each piece and its publishing history—a task complicated by the fact that most of these works were not only published anonymously but also given false title pages to conceal their true place of origin. The result is to show in a very precise fashion how even the most important political treatises that emerged from the religious conflicts—François Hotman's *Franco-Gallia*, Theodore Beza's *Du droit des magistrats*—were influenced by circumstances and slanted toward the audience whose support they were intended to win. This is not new. The theories of constitutionalism and resistance that were the major contributions of Hotman, Beza, and their peers have always been recognized as polemical responses to the conflicts of their time. What is interesting is the way that Kingdon has put the story together; his contribution is to show just how the polemicists transformed events as they directed their appeals to first one and then another audience.

Kingdon identifies three distinct purposes in the propaganda generated by St. Bartholomew's Day. It was intended first to keep alive the memory of the massacres by telling the world about them "in all their gory detail"; second, "to internationalize further the conflict of which the massacres were an episode"; and, third, "to challenge received ideas about the very nature of contemporary government"—to argue that the massacres had been caused by a government that "had gone berserk and become a tyranny" and to urge that this government be reconstituted so as to "protect the rights of subjects" (p. 5). The evolution from the first purpose to the last is exemplified by the change in tone between Goulart's initial description of the massacres, which stresses Gaspard II de Coligny's exemplary piety and was intended to stir Protestants to defend their threatened faith, and the last pamphlet in the collection, in which religion is scarcely mentioned. It focused rather on the villainy of Catherine de Medici and was intended to foster an alliance between the Huguenots and Catholic malcontents in rebellion against the Crown. Within these larger purposes, Kingdon shows how Protestant pamphleteers directed their propaganda to a variety of lesser aims. They worked, for example, to cut off the flow of Swiss and German mercenaries into France, to influence the election of a Polish king, to destroy a Scottish queen, and to encourage the calling of the French Estates General. The material presented here is fascinating, and, in his use of it, Kingdon reveals an exceptional understanding of the complex intrigues that made the French religious conflicts the stuff of both high tragedy and base melodrama.

My one criticism is that, in the brief passages in which he gives background on the events of St. Bartholomew's Day, Kingdon is too quick to accept the same Protestant sources whose mythmaking tendencies he elsewhere dissects. In attributing to Catherine de Medici the primary responsibility for the assassination of Coligny (p. 34), for example, he accepts an interpretation of events that deserves to be challenged. In fairness, however, Kingdon's version of events is the standard one, and his purpose is to reveal not the truth about St. Bartholomew's Day but rather its myths.

BARBARA B. DIEFENDORF
Boston University

JOSEPH BERGIN. *Cardinal de La Rochefoucauld: Leadership and Reform in the French Church*. New Haven: Yale University Press, 1987. Pp. viii, 302. \$32.50.

In the introduction to his study of François de La Rochefoucauld, a long-neglected figure of the French Counter Reformation, Joseph Bergin declares that he has not written a biography but a study in the twin themes of leadership and reform. The first five chapters discuss La Rochefoucauld's career as bishop of Clermont, cardinal, diplomat in Rome, bishop of Senlis, and Grand Almoner of France from the perspective of the diocesan activities shaping his thought in order to provide insight into his goals as a leader of the French Counter Reformation. The last five chapters describe La Rochefoucauld's efforts to put his ideas into practice as papal commissioner for the reform of the old religious orders in France from 1622 until his death. Dissatisfied with existing studies because they omit the darker side of the Counter Reformation—its failure to reform the old orders—Bergin has looked at reasons for La Rochefoucauld's successes and failures in reforming the Augustinian canons-regular, Cluny, Cîteaux, and the Mathurins. The size, numbers, and wealth of the old orders meant that diocesan and monastic reform were inseparably intertwined. Bergin challenges the established view that the true architects of the Counter Reformation were the middle and lower clergy, and in doing so he has restored the role of the upper clergy.

He has also written a study of the classic political problem of how to reform an entrenched bureaucracy—how to accomplish real change in the face of determined resistance by an entrenched administrative elite, a problem faced by Mikhail Gorbachev today. La Rochefoucauld was successful at reforming the Augustinian canons-regular because they were an order without central institutions or a corporate structure. Each house was a small, self-governing republic dominated by an

elite from local families. For this reason, La Rochefoucauld insisted on no outside or lay appointments of canons and no outside or secular benefices for canons. He also insisted on a central novitiate: all novices were to be sent to one house for training. Although the elite were loath to relinquish their rights to hold and nominate to outside benefices and to receive and profess novices, La Rochefoucauld persevered on a house-by-house basis and had reformed thirty-two houses of canons-regular by 1640.

His failure to reform Cluny and Cîteaux was largely the result of the highly centralized, institutionalized structure of these orders. Cluny had been in the gift of the Guise family since 1485 and had endured a long line of incompetent Guise abbots general. Cîteaux had fortunately escaped this fate. Although there was an abbot general, actual control of the Cistercian order was divided between the four powerful abbots of Clairvaux, La Ferté, Morimond, and Pontigny. La Rochefoucauld attempted to reform Cluny and Cîteaux on a house-to-house basis without considering the centralized, powerful interests he faced, a serious error in judgment, and so he failed. The Mathurins were a distinct branch of the canons-regular founded in the twelfth century for the purpose of redeeming Christian captives from Islam. The Mathurin elite resisting his reforms used the complex system of legal appeals against him: they formally appealed to Rome in 1638 against his proposals, which were quashed in December 1643 as seriously infringing the constitutions of the order.

Bergin remarks in conclusion that probably the most universal assumption about the Catholic church, past and present, is that it is a hierarchy in structure and operation. The French church in the sixteenth and seventeenth centuries, however, had comparatively few institutional sources of unity and authority. It was really a federation of more than a hundred episcopal republics over which archbishops, cardinals, and bishops exerted authority through personal ability, thus making the episcopal role in the Counter Reformation significant. Leadership in the church was personal, diverse, and divisive. The French clergy had an aversion to leadership from a single source.

This is an excellent book that makes an important contribution to our knowledge of the French Counter Reformation and at the same time offers broad insights into the administrative and structural characteristics of the early modern French church. It rescues from obscurity a long-neglected leader of the French Counter Reformation who was well-known and widely influential in his own time and explains the intellectual background of his reforms. The author remarks that the internal

politics of religious orders "rarely make exciting or cheerful reading," and he is right. In general, however, his book is beautifully written, lucid, and clear: he has handled a complex and potentially dry subject with style and grace. The archival research on which the book is based is impressive and painstakingly meticulous. Bergin's technical expertise reminds me of Alfred Brendel, who is known as "a pianist's pianist" because of his technique: Bergin may be a historian's historian because of his. He has written a very fine book.

SHARON KETTERING
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JAMES PRITCHARD. *Louis XV's Navy, 1748–1762: A Study of Organization and Administration*. Montreal: McGill-Queen's University Press. 1987. Pp. xiv, 285. \$32.50.

Inspired by David Baugh's *British Naval Administration in the Age of Walpole* (1965), this sophisticated and tightly argued work by a Canadian scholar allows historians to compare French naval administration, men, and material at midcentury. Like Baugh, James Pritchard provides a detailed description of the central administration (the secretaries of state for the navy and their bureaus), the civilian administrators (officers of the pen), naval officers (officers of the sword), manpower, the operation of arsenals, shipbuilding, provisioning, the cannon shortage, and the navy's complex fiscal picture.

The defeat of the French navy in the Seven Years' War traditionally has been explained by poor organization, corrupt mismanagement, ministerial neglect, and military unpreparedness. Pritchard takes us beyond these generalities to a deeper and more accurate characterization of the French naval system by placing it within the economic, social, and political context of absolutist France. He argues convincingly that the fundamental weakness of the French navy lay not in corrupt officials but in underfunding.

Money and credit, Pritchard reminds us, basically determined French naval power. With minimal funding the navy remained understrength with inadequate stores, limited preparedness for combat, inexperienced officers, and an apparent shortage of seamen. Since the financial system relied on private financiers and public confidence, naval officials had no real role except the preparation of spending estimates. Thus, naval policy basically was dictated by outside forces.

Further, the navy was limited by Bourbon administrative processes. Pritchard downplays the traditional hostility between civilian and naval of-

ficials (sword and pen); clashes with outside government agencies were more significant. More important, the French naval administration was not highly centralized. It lacked an effective chain of command, maintaining weak links between the center and peripheral officials. Pritchard reminds us that it is a mistake to see absolutism as a centralized will rationally choosing options. The king and the naval secretary had little knowledge of naval affairs. Heads of bureaus remained specialists whose influence depended on their relationship to the rapidly changing ministers (the navy had seven secretaries of state in this fifteen-year period). Information flowed without real coordination aimed at effective management.

Moreover, aristocratic naval officers never achieved, nor really desired, an effective voice in naval administration. Although personally courageous, these officers did not develop a professional esprit de corps backed by experienced naval training. As a result, the government lacked an expert group who could recommend naval strategy. Without a realistic naval strategy, the administration preoccupied itself with matters of money and material, without hope of achieving success.

The product of some ten years of research, the book rests on the basic secondary literature and relies heavily on archival material, both in Paris and in the marine archives at Rochefort, Brest, and Toulon. An appendix devoted to the difficulties of using financial documents in determining navy funding and expenditure is included.

This important work is essential for understanding French naval history and has wider implications for Bourbon administration and English-French overseas rivalry. Highly recommended, this book will remain one of the crucial studies of the French navy under the *ancien régime*.

DOUGLAS CLARK BAXTER
Ohio University

MARTYN LYONS. *Le triomphe du livre: Une histoire sociologique de la lecture dans la France du XIX^e siècle*. Paris: Promodis. 1987. Pp. 302. 250 fr.

Historians live by the printed book; until recently, however, they rarely treated it as a subject for historical research. That has now changed, and readers of Martyn Lyons's study will recognize—in the use of quantitative data, social history, anthropological sensitivity, and some Foucauldian vocabulary—how much our current conception of the topic owes to recent trends in historical writing. The fact that the book industry has been more systematically explored for the eighteenth than for the nineteenth century makes Lyons's essay all the more welcome. In part a synthesis of scholarly

work on the culture of the book in nineteenth-century France, the study incorporates not only the author's aperçus and suggestions for further work but also some impressive independent research.

The framework is very broad, for Lyons really does see, and to a certain extent explore, the varied possibilities of his topic. He writes thoughtfully about the connections between formal and popular culture, the development of the printing industry as an aspect of capitalism and industrialization, the spread of literacy, the content and appeal of romanticism, the values associated with individualism, the image of society projected by novels, the traffic between Paris and the provinces, and the effect that state policies had on the commerce in books. Such coverage is bound at times to seem a bit sketchy, and one could wish that this were a lengthy French *thèse* in which Lyons's imaginative use of evidence and his sensible skepticism were applied to a fuller set of sources.

As it is, he has many suggestive things to say. He notes that exchanges flow in all directions among folk tales, almanacs, newspapers, and novels—a fact that leads to some doubt about concepts of social control. Popular and bourgeois mentalities, he shows, were not always in conflict. While accepting the usual view of the self-interest behind the moralizing campaigns of bourgeois reformers, he adds further evidence of their limited success. He argues that the increased production of books in the nineteenth century (from some thousand titles a year on the eve of the revolution to three to four thousand a year at the end of the Empire, seven to eight thousand during the Restoration, and twelve thousand a year in the Second Empire—a figure that then rose very little) marks the shift from a segregated elite culture to a more homogeneous national culture built around popular works like the *Mystères de Paris* and the novels of Jules Verne. The decline of colportage, a result of economic change and official repression during the Second Empire, was also both a cause and an effect of cultural change. Discussions of the publishing industry (including production and distribution), Belgian competition, government regulation, and literacy—all largely based on secondary work—describe the mechanisms for this cultural transformation. Its significance is further explored in a brief final chapter that considers changes in the way books were packaged (including a discussion of the connection between illustrations and accompanying text, based mainly on editions of Sir Walter Scott's *Waverley*) and that reflects on how artists portrayed the act of reading in cartoons and paintings (usually an individual, contemplative act and one most often performed by women)—evidence that leads Lyons to question

whether reading aloud was ever so important among the lower classes as we have assumed.

All this sensitive, sensible, tentative, and incomplete discussion serves as a setting for four chapters based almost entirely on Lyons's original research. Carefully presented discussions of quantitative data, these chapters analyze: French best sellers from 1816 to 1850, all the editions of Scott in the same period, the number and location of libraries established for the people in the second half of the nineteenth century, and the number and location of bookstores licensed from 1851 to the 1880s. For all their limitations, of which Lyons is very much aware, such rich data can be used in many ways. He seeks out broad patterns in the content of popular books (through *précis* that are not very satisfying) and in statistics of their availability (handled with great caution).

He reveals the prominence of French classics and devotional works among nineteenth-century best sellers, especially during the Restoration. (Fénelon and Jean de La Fontaine maintained their popularity in the provinces.) He notes the rising taste for novels, history, and such works as *La cuisinière bourgeoise*, published in editions that tended to become larger as the years went by. (Most printings of Scott's novels had been between one and two thousand copies.) The books of self-improvement favored by the Société Franklin and others active in the movement to create libraries for the people were less popular; but then the clientele, Lyons suggests, were more often students, housewives, soldiers, and white-collar workers than the proletariat for whom those institutions had been intended. The lower middle class, more than the workers (who made up from one-tenth to one-third of the customers), were drawn into the national culture of the book. Analysis of prefects' reports on the number of licensed bookstores reveals the early lead of the northeast and of cities, especially established administrative and cultural centers, as well as the spread of bookstores along communication routes and their rapid increase in total number until even southwestern France and Brittany were well linked to a national network of distribution. This picture of France's cultural geography is a familiar one, but the close analysis provides Lyons with a strong basis for arguing that modernizing ideas began their rapid penetration into the countryside during the 1850s rather than a generation later.

RAYMOND GREW
University of Michigan

WILLIAM LOGUE. *From Philosophy to Sociology: The Evolution of French Liberalism, 1870–1914*. Dekalb:

Northern Illinois University Press. 1983. Pp. xiii, 278. \$22.00.

Jean Dubois in his fundamental *Le vocabulaire politique et social en France de 1869 à 1872* (1962) observed that in 1871 the use of the adjective "liberal" created a lexical system in which "total confusion" prevailed (p. 76). William Logue's history has the great merit of establishing that both before and after the Commune the idea of liberalism possessed coherence, continuity, and depth. His history examines the generations of philosophers, moralists, and, in time, sociologists for whom the promises of liberalism were central to France and the Third Republic's political and social future. Disciples of Victor Cousin such as Paul Janet, Emile Caro, and Adolphe Franck are credited with creating a sustaining moral discourse adequate to test the worth of France's political and social institutions.

Yet by the eve of 1870 the *Cousinians* belonged more to France's past than to the nation's future—a disjunction that permits Logue, whose own liberalism is never in question, to celebrate the importance of Charles Renouvier. His *Science de la morale* (1867) is uncritically praised as "the unrecognized masterpiece of nineteenth-century philosophy" (p. 51): a work Logue considers as uniting the implicit moral postulates of the French revolution and the values of a democratic republic and an achievement he judges added urgency to the development of a universal secular moral education, explicated by advocates having the acumen, sensibility, and fervor of Ferdinand Buisson and Gabriel Séailles.

The pragmatic and missionary success of these theorists is viewed as, in turn, enhanced, modified, and built upon by stellar academic sociologists-philosophers having the audience and stature of Gabriel Tarde, Alfred Fouillé, and Emile Durkheim. France's long nineteenth-century search for a perfect Gallic categorical imperative is fittingly closed by Logue's attention to the originality of the Durkheimian Célestin Bouglé's use of sociology to examine the limits and crisis of liberalism at the beginning of the twentieth century.

Some historians may consider Logue's history less successful than that of Claude Nicolet's *L'idée républicaine en France (1789–1924)* in uniting the Third Republic's moral priorities and actual polity. Other historians may continue to wonder how much the sad antihero of Paul Nizan's novel *Antoine Bloyé* (1933) was helped in 1879 by his prize copy of Jules Simon's *Le Devoir*.

EDWARD T. GARGAN
University of Wisconsin

JEAN DOISE and MAURICE VAÏSSE. *Diplomatie et outil militaire, 1871–1969*. (Politique étrangère de la France.) Paris: Imprimerie Nationale. 1987. Pp. 566. 190 fr.

This is the first volume of a projected three-volume series titled *Politique étrangère de la France, 1871–1969*, under the editorship of Jean-Baptiste Duroselle. The next volumes will consider the impact on French foreign policy of economic and ideological factors, respectively. Together, these will present a welcome synthesis of scholarly work on the history of French foreign policy from the collapse of the Second Empire to the beginning of the Fifth Republic. This volume analyzes the *outil militaire* (the personnel, armaments, budgetary appropriations and strategic planning of the land, sea, and air forces) and its relationship to French foreign policy in the century following 1871. Jean Doise, a specialist in fortifications, wrote the first half of the book (1871–1918), and Maurice Vaïsse, who specializes in modern French diplomatic and military history, authored the second half (1919–1969). Each part has its own tone and orientation.

Doise's contribution is decidedly technical, focusing on weapons development and deployment, ammunition, fortifications, recruitment, naval development, and the evolution of grand strategy culminating in "Plan XVII" of 1914. This author's discussion of diplomacy is cursory and isolated in short sections of chapters. His approach, however, yields some interesting insights. Doise demonstrates that, by maintaining their fortifications along the eastern frontier while neglecting those in the north, the French inadvertently encouraged Germany to base its strategy on invasion through Belgium. Thus French security rested on a weak neighbor with mediocre fortresses and an insufficient army. Ironically, the intellectual failure of French military planners may have assured ultimate victory. A German invasion of Belgium seemed incomprehensible to the French because it would force Great Britain to intervene, thus transforming a European continental war, "for which Germany had carefully prepared with every chance for victory, into a world-wide maritime struggle which it would probably lose" (p. 177).

In the second half of the book, Vaïsse's chapters are less technical but offer a more sophisticated interweaving of diplomatic and military policy. In the interwar period, he finds little coordination between a French diplomacy based on eastern alliances and the League of Nations (a policy charted in the early 1920s) and an increasingly defensive military strategy calling for entrenchment behind the Maginot line—a contradiction that increased the vulnerability of the Third Republic.

Given the military and political collapse of 1940, the humiliation of 1954 in Indochina, and the frustrating postwar years as a pawn in the cold war, the French set as their goal the recovery of national independence. For Vaisse, this explains Charles de Gaulle's insistence on developing an independent nuclear *force de frappe* and his willingness to relinquish control of Algeria. De Gaulle saw the continuing stalemate there as a detriment to national prestige that increased France's dependence on the Atlantic alliance. Vaisse concludes that the Gaullist period produced a harmonious adaptation in which "the coordination between exterior policy and military policy was ideally realized" (p. 492). The challenge for the future, given the continuing question of East-West equilibrium, is how best to coordinate a policy of national independence with French participation in the defense of Europe.

Despite the authors' differences in style and substance, both cite an impressive array of literature in French. The book is handsomely produced and accompanied by photographs, maps, graphs, and charts. But there are problems, which one hopes will be considered before publication of the next volumes. First, there is no bibliography, making it tedious to track down information on works cited more than once. Second, there is a curious lack of reference to studies in languages other than French. Surely, the work of experts such as Michael Howard, Alistair Horne, Samuel Williamson, Robert J. Young, and Jolyon Howarth deserves consideration. Finally, a lengthy work such as this needs a thorough index (rather than a cursory index of proper names) if it is to reach maximum potential as a scholarly tool.

WILLIAM I. SHORROCK
Cleveland State University

MARC FERRO. *Pétain*. Paris: Fayard. 1987. Pp. ix, 789. 150 fr.

In 1940, the French spent thirty-nine days fighting the Germans before they laid down their arms. They have spent more than a half-century since then fighting each other about it. The role of Marshal Pétain in the armistice of 1940 and his responsibility for the conditions of defeat and for France's sufferings in the years that followed have been the subject of much debate. Marc Ferro, editor of the *Annales*, who dedicates this book to Fernand Braudel, has a great deal of ground to cover. He has done his homework. Ferro has plumbed public and private archives in Paris, Montreal, Ottawa, Stanford, and Moscow and all of the published documents that he could get; he has considered the arguments and "revelations"

on all sides. And he gives us a judicious, sensitive, scholarly account of Pétain from 1940 to 1945, of the marshal's last years, and of the mythology that has sprung up around him.

We are led through the battle of France, exposed to the growing resentment against the English—perceived as eager to fight to the last drop of French blood as in the First World War—initiated in the defeatism and panic gaining on all sides as the French took to the roads to find refuge within France. Latent anti-Semitism and explicit antiparliamentarism swelled the rancor against those who wanted to carry on fighting, until the Third Republic, born in one defeat, foundered in another. There was no conspiracy in 1940. As Ferro makes clear, Pétain (and Pierre Laval) did not have to kill the republic: its representatives, almost unanimously, scuttled it for them.

Vichy's politics are minutely etched, from recurrent crises to concessions, betrayals, reconciliations—one more depressing than the other. Ferro confirms the crab-basket aspect of Vichy, the love-hate relationship between Pétain and de Gaulle, the symbiotic affinities of Pétain and Laval, the respect (amounting sometimes to adoration) in which most French long held the grand old man—witness all of the Philippes and Philippines of those braid-worshipping times. He depicts the dreadful effect of collaboration on collaborators, many of whom put their fingers in its deadly works to help save fellow Frenchmen, only to destroy both them and themselves. He quotes Marshal Runstedt's terrible report when the Germans took over the unoccupied zone in November 1942: "The French army is loyal, the police assiduous" (p. 444). And, with masterly discretion, Ferro touches on the final decadence of a man more striking for sporadic vigor than for creeping senility. One has to agree with Laval (who did not bear him in his heart) that Pétain loved France and sacrificed his glory to it. But one concludes that, "halfway between sacrifice and treason" (p. 635), Pétain loved himself even more than he loved France and sacrificed his glory to his vanity.

A number of new documents and of new readings of known documents place some events in new perspective. Inevitably, though, Ferro's narrative is more exhaustive than it is novel. Sometimes exhausting, too. His blow-by-blow account of a few years that began with catastrophe and ended worse will prove of real use to specialists who will pick up new bits of information and also refresh their memories. Civilians who delight in detail will also delight in the 728 pages of detail offered here. They may also feel after a while that they miss the forest for the trees.

Dismissing such quibbles, I enjoyed the book and admire it. My one criticism is that it is not

about Pétain but about Pétain the public person, after 1940. But in 1940 Pétain—born on my own birthday but a little before I was—was eighty-four years old. For *Pétain avant Vichy* we still have to turn to Henri Amouroux. The experiences and events that made the man, the savior, and the failure receive scant attention: half a dozen pages at most, scattered here and there. In a book entitled simply *Pétain*, we could surely have been told more than that.

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ADRIAN SHUBERT. *The Road to Revolution in Spain: The Coal Miners of Asturias, 1860–1934*. (The Working Class in European History.) Urbana: University of Illinois Press. 1987. Pp. 183. \$22.95.

Adrian Shubert's study of coal miners in Spain's Asturias region falls into two distinct parts. The first four chapters study economic and social conditions in Asturias from the late nineteenth century until the end of World War I. This section of the book provides Shubert a firm basis for rejecting explanations of the Asturian revolution of 1934 that are based on the status of miners as a group separated from the larger community. Shubert shows that the Asturian mining community was neither isolated nor homogeneous. Miners in Asturias were generally mixed in with the rest of the population. Any solidarity the mining experience might have fostered was shattered by generational splits, differences between those who worked inside and outside the mines, and cleavages between native Asturians (who generally combined work in the mines with agricultural work) and immigrant full-time miners.

The second half of the book is a narrative history of the labor movement in Asturias from 1873 to 1934, concentrating on the period 1920 to 1934. Shubert finds the roots of the Asturian revolution of 1934 in the long crisis that the coal mining industry and the principal miners' union, the Sindicato de Obreros Mineros de Asturias, entered at the end of World War I. He identifies five significant developments: the ongoing economic crisis of the coal industry, the inability of the unions to shield their members from the social and economic consequences of the industry's crisis, the failure of the Second Republic to provide a remedy for the miners' economic distress, the pessimism and fear created by the rise of fascism in Europe, and the temporary end in 1934 of ideological hostility between Socialists, anarcho-syndicalists, and Communists. Disillusionment with the Second Republic's failure to deal success-

fully with the miners' problems, according to Shubert, converted economic grievances into revolutionary clashes.

Shubert's analysis of the social and economic climate of Asturias, although flawed by a somewhat dogmatic stress on class conflict, shows that the revolution of 1934 cannot be explained in terms of a static, isolated, and homogeneous mining community. His explanation of why a revolution actually broke out in Asturias in 1934, when Socialist attempts at uprising in other parts of the country were failures, is less satisfying. Economic hardship and miners' disillusionment with the failure of the republic to solve their problems do not seem to explain why a revolution took place only in Asturias when other regions faced equally difficult economic crises and when workers in other parts of the country were presumably also disillusioned with the republic.

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STANLEY G. PAYNE. *The Franco Regime, 1936–1975*. Madison: University of Wisconsin Press. 1987. Pp. xvii, 677.

This is Stanley G. Payne's fifth major monograph on twentieth-century Spain. His new book is well organized and has good maps and pictures and excellent statistics. It is cautiously written and demonstrates prodigious research. *The Franco Regime* confirms that Payne will remain the leading U.S. authority on twentieth-century Spain for some time to come.

Few have consulted the wide variety of sources that Payne has. He is a master of both Spanish- and English-language materials and uses German, Italian, and French studies to a lesser extent. Both footnotes and sources are informative, but the reader should be cautioned that Payne gives fuller treatment to conservative scholarship on Spain than he does to Marxist and liberal works.

This subtle book is not for beginners, but it will be a reference work essential for every twentieth-century European historian. Although hundreds of books have dealt with the Spanish Civil War, 1936–39, Payne's analysis (chapters 6–11) is the freshest account yet.

Nevertheless, despite the masterly writing, I must dissent from the apparent underlying philosophical and political theses, especially those in the introductory chapters. Although capturing well the conservative spirit of 1959 in both America and Spain, Payne tends to miss the emotional tone of the years from 1936 to 1939. According to Payne, social revolutionaries and their liberal collaborators were and are naive, and the body of

Anglo-American scholarship that existed in 1959 on the 1936 Spanish crisis was misguided. The name "Loyalist," to which Republican partisans were wedded in the 1930s, does not appear in this book. Spanish counterrevolutionaries and fascists, on occasion lumped together with conservatives, are sometimes excused from their sins. Terms like patriotic, heroic, modernizing, and middle class, although they do not appear in the index, often are used to describe the Spanish fascist movement.

On the other hand, in chapter 11 on "The Repression," Payne anticipates the left-wing reviewer by graphically portraying Francisco Franco as the ruthless tyrant he could be. Also Payne's conclusion shows close parallels between Benito Mussolini's Italy and Franco's Spain (pp. 628–29), which the main text plays down. The reader may find it difficult to detach Payne's main thesis from his defensive cover.

Franco, who aspired to be a totalitarian, was no Hitler who could mesmerise the public. Administratively Franco did not have a Hess, a Bormann, or a Himmler. In foreign policy Franco's conquest of Tangier hardly equaled Hitler's takeover of Europe by 1941.

Payne brilliantly demonstrates that intrigue against Franco from military and party subordinates occurred to a far greater degree than it did in the Third Reich from similar groups opposed to the Führer. As a result Franco reshuffled his cabinet frequently and did not experience the feeling of internal security that Hitler enjoyed. An appendix listing the many Franco cabinets would have helped the reader.

Still, the Generalissimo from 1936 to 1942 was essentially more fascist than Payne generally admits. By using subordinates to advance his cause through military force, Franco, in April 1937, officially became a fascist leader, the *jefe nacional* of national Spain and the *caudillo* of the unified fascist party, the FET y JONS. In July 1936 when Franco decided to join the military revolt against the Spanish constitutional republic, he already was demonstrating sentiments that were more fascist than they were part of any other ideology. His Catholic and absolutist monarchist allies did not control Franco. Neither Hitler nor Franco ruled under a constitution, and until 1943 the Spanish dictator was confident of Hitler's victory against both liberalism and Communism.

Franco was not at that time an "authoritarian" (p. 174), as Payne confusedly puts it. The problem with his concept of authoritarianism is that it is vaguer than fascism, which admittedly also has its difficulties. According to Payne, there is "fascist type authoritarianism" and the "authoritarian corporate state" (p. 46). Payne adds Catholic authoritarianism, military authoritarianism, bureaucratic

authoritarianism, and "traditional" conservative authoritarianism to his thesis.

As a specialist on the 1936–39 era, I am less critical of the last half of *The Franco Regime*, which covers the years 1942–75. From the time of the American landing in North Africa in 1942 until 1959 when autarky was abandoned, the regime gradually became, in my opinion, primarily a military dictatorship. Franco kept control in his own hands while promising eventual restoration of the monarchy. From 1959 to his death, the Generalissimo actually returned to his youthful conservative views. After 1959, however, Franco permitted members of his cabinets to liberalize the vestigial remains of fascism, despite his own personal inclinations.

Payne's major quest in this book—to describe "authoritarian unity" in Spain from 1917 to 1975—tends to emphasize a consistency in Franco and in his regime that both lacked at the time. Then, like an abstract expressionist artist, Payne paints over his basic outline and in the conclusion partly plays down Franco's "authoritarianism." He includes his own critical review, in that concluding chapter, which portrays the three phases of Franco's leadership (1936–43, 1943–59, 1959–75) that I have outlined here.

ROBERT H. WHEALEY
Ohio University

SHEELAGH M. ELLWOOD. *Spanish Fascism in the Franco Era: Falange Española de las Jons, 1936–1976*. New York: St. Martin's. 1987. Pp. vii, 207. \$32.50.

During the past three decades the literature on Spanish fascism, especially on the Falangist party and National Movement, has grown considerably with the addition of works written not merely in Spanish and English but also in French and German. The British scholar Sheelagh M. Ellwood has now brought out a brief new general history of Falangism from its origin to the immediate aftermath of the Franco regime.

An earlier version of this study was published in Barcelona in 1984 under the title *Prietas las filas: Historia de Falange Española, 1933–1983*. Although Ellwood does not refer to the earlier publication in her introduction, a brief comparison indicates that the two editions are not equivalent. The main emphasis in both is on the years after the end of the Civil War, which have been studied much less, both inside and outside Spain, than have the 1930s. In the English edition, Ellwood treats the decade of the 1930s, and particularly the years before the Civil War, more briefly than she does in the Spanish version and thus gives greater propor-

tionate weight to the later period. This is in general a sensible approach, because the author has little to add to the earlier part and more effectively maintains her focus on material where she has more to contribute. Conversely, the abbreviation results in largely passing over some important aspects, such as the Falangist role in the political violence that figured prominently in the coming of civil war.

Given the unavailability of the archives of the party, which "disappeared" during the democratization of Spanish government, major new sources are not readily available. Ellwood's primary contribution stems from extensive interviews among party veterans, which have turned up at least a certain amount of new information. She has also read widely in the lengthy corpus of Falangist publications. Her account does not especially alter existing knowledge of the principal developments of the 1940s and 1950s, before the full paralysis of the party, but she does occasionally add an interesting new item.

The chief contribution of this book, however, lies in part 4, titled "Falange Idealized," which occupies nearly one-fourth of the book. This section consists of the first general account of various expressions of dissident Falangism from the end of the Civil War to the first phase of democratization. This material appeared in the Spanish edition, but Ellwood has now provided the only synthesis available in English of the varied aspects of this phenomenon. Although dissident Falangism, whose adherents severely criticized and often rejected Franco's official National Movement, was not truly a major factor in Spanish political development, it was part of the peculiar political half-life of the Franco era and merits some attention.

This study does not significantly change our understanding of the history of Falangism, but it is carefully done, generally accurate and objective, and provides a useful, brief summary of the peculiar trajectory of Falangism from its beginning to the official dissolution of the movement in 1977.

STANLEY G. PAYNE
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VIRGINIA HIGGINBOTHAM. *Spanish Film under Franco*. Austin: University of Texas Press. 1988. Pp. xiii, 160. Cloth \$20.00, paper \$9.95.

Although the title may indicate a highly specialized study, Virginia Higginbotham's monograph addresses a topic that should interest an array of students of modern history. In surprisingly few pages, she sketches the development of Spanish cinema with particular attention to the period from 1936 to 1975, supplemented by synopses of

the major films produced in Spain for domestic audiences during the era of Francisco Franco. The volume concludes with an extensive bibliography, a unique filmography, and a useful index.

Higginbotham announces in the preface that this work is "not a comprehensive history"; therefore, those seeking a definitive tome on the evolution and implementation of cultural policy under the Franco dictatorship will find the book a disappointment. The author's approach is less narrative than it is episodic; she devotes more attention to encapsulating the plot lines of productions by such noted directors as Juan Antonio Bardem, Luis García Berlanga, and Carlos Saura than she does to analyzing the authoritarian ambiance in which they functioned. Too often Higginbotham is content to rely on the reader's understanding of the label "fascist" rather than detailing the regime's transmogrification over nearly four decades.

Compared to the volume's valuable contents, however, these shortcomings are marginal. The author maintains that the victorious Nationalists captured a nascent film industry, imposed regulatory machinery on it that the regime borrowed unabashedly from Fascist Italy, and proceeded for the next two decades to sustain a cinema that depicted only "historical extravaganzas," religious subjects, and "folklore musicals" (p. 18). Within this official context, however, there emerged what Higginbotham terms the "Franquist aesthetic," an "ironic film style," through which talented directors sought to address their audiences obliquely. Its characteristics were "abrupt cuts, obscure images, and juxtapositions, unexplained actions, and incorporation of horror devices from surrealism . . . not easily accessible to unprepared viewers" (p. 120).

Luis Buñuel's influence in the creation of this genre is manifest, and, although the author acknowledges his genius, she also urges Buñuel's successors in Madrid and Barcelona to amend his aesthetic by making the nation's modern experience accessible to a broader public. Her analysis is a welcome addition to the only other recent treatment in English of the subject, Peter Besas's *Behind the Spanish Lens* (1985), and is a timely companion to the University of Oregon's newly created videocassette archive of Spanish feature films.

DOUGLAS W. FOARD
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LENNART G. SVENSSON. *Higher Education and the State in Swedish History*. Stockholm: Almqvist and Wiksell International. 1987. Pp. 254. 175 KR.

This synthetic study, based mainly on secondary sources in Swedish and published originally in this

English translation, appears to be aimed at a broad audience of historians and sociologists of education. Lennart G. Svensson describes his "over-riding theme" as "the nature of historical change in state technologies" (p. 111), which he examines with regard to the "feudal, liberal, and rationally planned university systems" (p. 7) in Sweden since 1600. Among the topics to which he devotes attention are the evolution of relations between the government and the universities, the changing positions of the individual faculties within the universities, the proliferation of specialized disciplines and professorships, the social origins of students in different fields, and the process of professionalization.

This general approach could have made the book as valuable a survey as Charles E. McClelland's *State, Society, and University in Germany, 1700–1914* (1980). Yet Svensson's work falls far short of such a standard, primarily because of a structure that leaves even the interested and conscientious reader reeling. Rather than presenting comprehensive pictures of the universities during the three major eras he delineates, Svensson takes each of his themes and subthemes through the entire period covered by the book, which involves dropping back at least twenty-four times from the late twentieth century to early in the seventeenth. The result is that references to decisive events such as the new university constitutions of 1626 are scattered throughout the book: because there is no index, earlier references are extremely difficult to locate when a topic resurfaces in a later chapter.

Two topics deserved more attention. Svensson discusses many intellectual and institutional influences coming from the German universities but never offers a clear conclusion about the extent to which Swedish universities followed foreign models. More important, the fight over the admission of women to higher education in Sweden and the distribution of female students among the various faculties are never mentioned.

The translation is awkward at many points and inconsistent at others: one monarch, for example, appears as both Charles XI and Karl XI. The book also suffers from numerous typographical errors; eight footnotes are omitted entirely in Chapter 11. One must regret that the interesting information and interpretations Svensson has to offer were not presented in a more readable way.

JAMES C. ALBISETTI
University of Kentucky

New York: Clarendon Press of Oxford University Press. 1986. Pp. 265. \$49.95.

Tom Scott has few equals as an expert on the economic and social facts of rural and urban life in southwestern Germany in the fifteenth and sixteenth centuries. In this long-awaited book, which was preceded by several thought-provoking and argumentative articles, he concentrates on the relations between the town of Freiburg and the territory it dominated, offering the result as a case study of a number of interpretive problems now troubling scholars.

Seen in the perspective of the town's ruling groups, the story of Freiburg is one of modest success, as the governing citizens overcame repeated challenges by artisan guilds and feudal lords, a process culminating in the town's ability to turn tables on peasant insurrectionaries who briefly occupied Freiburg in 1525. Scott makes survival in an age of economic ups and downs, rural discontent, religious strife, and church-state rivalry sound like no mean accomplishment. Freiburg's modest economy of artisans and retailers remained sound. Civic unity was preserved through a constitution—evolved in a process of give and take between 1460 and 1525—that reflected a basic citizen consensus, leaving out only the meanest groups gathered in the wine growers' guild, the only urban faction infected by the rebellion of 1525. Within its region, the town was, and acted like, a little territorial state. Relations between clergy and laity remained largely civil. No Reformation of any consequence occurred. Suspicious of outsiders and stolidly conservative, sixteenth-century Freiburg was the very model of a "home town" in the Holy Roman Empire.

Scott's description of these developments, given in rich and concrete detail and in a running dialogue with the abundant sources (notes are where they belong, at the foot of each page) is an impressive achievement. But there is more. The book contributes assertively to two scholarly debates: the town-and-country controversy, and the discussion over the events of 1525 as the "revolution of the common man." On town and country Freiburg's experience argues persuasively for the interpenetrative and interactive relations of rural and urban societies. Friction between town and countryside resulted not from differences in their respective characters but from close mutual involvement. "As its economic centrality contracted in the face of rural competition, Freiburg's hostility toward its hinterland increased: as its political centrality expanded with the acquisition of a feudal territory the countryside's antagonism towards the town grew deeper" (pp. 234–35). Urban cen-

TOM SCOTT, *Freiburg and the Breisgau: Town-Country Relations in the Age of Reformation and Peasants' War*.

ter and urban countryside formed a single unit, not separate entities.

Scott's major argument is aimed at Peter Blickle's concept of a popular revolution resulting from the juncture of peasants and disaffected townspeople in the outbreaks of 1525. Scott asks: did ideas of "Christian liberty," "divine justice," and "communal association" act as the ideological glue to cement otherwise fractious rural and urban groups? Here, he says, is the detailed regional investigation that proves that they did not. At pains to demonstrate that Freiburg offers a not atypical instance, he shows that the peasant cause aroused no significant actions of sympathy in any urban group. Burghers failed to respond to peasant slogans that could not bridge the divisions of interest separating town and country. Passionately unideological, Scott finds that circumstances, notably economic turns, shaped the relations of city and country people: when alliances of peasants and burghers came about, they served particular and short-term objectives. There were no common interests transcending the requirements of the moment. Scott hints that, even for the peasants, appeals to God's law had no more than instrumental value. And he ends with the general suggestion that the prominent role usually given to ideology in the interpretation of revolution ought to be reduced.

Scott's powerfully put argument must now be faced by those who have found Blickle's synthesis so satisfying as a conceptual frame in which to understand the crisis of 1525. The lively debate his and Blickle's works have set in motion is one of the reasons why Reformation scholarship is so exciting these days.

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SUSAN C. KARANT-NUNN. *Zwickau in Transition, 1500–1547: The Reformation as an Agent of Change*. Columbus: Ohio State University Press. 1987. Pp. 299. \$29.50.

Susan C. Karant-Nunn's second book studies the Saxon city of Zwickau—a cloth town of seventy-five hundred people on the edge of the Erzgebirge—before, during, and just after the Protestant Reformation, roughly 1480 to 1550. Based largely on archival materials from Zwickau and Weimar, the book presents two fronts to the field of German Reformation history. First, this extremely skilled, thoughtful, and gracefully written monograph on the Reformation in a single city contributes to the burgeoning literature on the German urban reformation. Second, this book breaks truly new ground in presenting not an-

other southern free city but a leading territorial city. In such cities, which housed the vast majority of German burghers, the Protestant religion developed not through a two-way struggle between the burghers and the oligarchical regimes but in a three-way confrontation that included the territorial prince. At Zwickau the prince was the Saxon elector, who had protected the excommunicated Martin Luther and thereby made the Protestant movement possible.

In three opening chapters, Karant-Nunn describes the structures of governance and economy in a city that had lived from the cloth trade and was by 1500 suffering from Leipzig's competition. The silver boom temporarily slowed Zwickau's decline but not the growth of oligarchy nor its political estrangement from the guilds. The critical confrontation came in 1516–17, just before the Reformation, and ended in a defeat of popular reform and a victory by the oligarchs, who were backed by the Saxon elector. This winning combination eventually sponsored the establishment and consolidation of the Lutheran Reformation in Zwickau.

Three more chapters describe the rise and fall of a spectacular popular movement, inspired by Thomas Müntzer and led by the artisan-reformers called the "Zwickau prophets," that gave way to the urban regime's strictly Lutheran Reformation from above. The popular movement of 1521–22 at Zwickau, Karant-Nunn argues, flourished because "the Reformation seemed to give a general license to act out against corruption and oppression in all their forms" (p. 243), and it gave Zwickau's artisans "the hope that they might now . . . return to what they thought a golden age of guildsmen's collective dignity" (pp. 242–43). In its place came a Lutheran reformation from above, which "enabled the oligarchs to realize their dream of nearly total control without a coup d'état—without openly rejecting traditional political forms and imposing alien ones" (p. 2). The change had serious consequences for all forms of popular culture, and "the great beneficiary of the Reformation era was the prince himself" (p. 245).

In her final chapters, Karant-Nunn describes the consolidation of Zwickau's Reformation and oligarchy and the growth of princely control after the Schmalkaldic War. Karant-Nunn herself believes that the Reformation was not a prime mover but a "catalyst" in a development prepared and shaped well before 1520. The Zwickau reform's triple movement—popular reform, oligarchical reform, princely control—supports all those who see in the German Reformation a transition from a failed popular reform to a successful authoritarian one. Peter Blickle's thesis of a "communal reformation" is one such idea. Another interpre-

tation is the thesis of "the reformation as early bourgeois revolution," which is found in East German historiography. Against this notion, a thread of polemic runs through Karant-Nunn's book.

This fine monograph illustrates how closely akin are most current visions of the social history of the German Reformation. All of them hold that the Reformation initially animated both sides of a tremendous struggle for the control of everyday life, although Protestantism eventually allied with the centralizing state. It all boils down to one's view of the relative importance of two struggles: the struggle of the Evangelical movement against the old religion and the struggle of Evangelicals for control of the new one. The weakest combination in this complex conflict was that of popular forces with Catholicism, an association that happened in many parts of Switzerland. By far the strongest combination was the alliance of centralizing temporal power with Protestantism. This very fine book demonstrates that in far East Elbia, at least, nothing could stand against this winning combination.

THOMAS A. BRADY, JR.
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HANS ERICH BÖDEKER and ULRICH HERRMANN, editors. *Über den Prozess der Aufklärung in Deutschland im 18. Jahrhundert: Personen, Institutionen und Medien*. (Veröffentlichungen des Max-Planck-Instituts für Geschichte, number 85.) Göttingen, FRG: Vandenhoeck und Ruprecht. 1987. Pp. 206. DM 48.

This volume contains seven essays, originally presented at a colloquium in Germany in 1982. Its introduction, composed by the editors, seeks to place the individual contributions within a larger interpretive context and to propose a program for the study of what the editors call the "process of Enlightenment." The term "process" is intended to include both static and dynamic components. Thus, the research program calls for the analysis of structural elements as well as investigation of the paths and means by which Enlightenment culture was diffused. It requires studies of representative individuals, central Enlightenment institutions, and the prevalent modes of communication. This program is ambitious and innovative. It demonstrates the increasing German reception of French and Anglo-American approaches to the social history of ideas as well as the German concern with theoretical models of process and structure. If implemented, it promises to enhance and partly redefine the traditional view of the

German Enlightenment. The editors emphasize the need to turn from more generalized statements about the Enlightenment to concentrate on regional and local studies. Only through them can the German Enlightenment be understood in its complexity.

Such an ambitious project is difficult to realize and certainly could not be exhausted by this relatively small collection of essays. Still, enough new material is presented to justify the editors' hopes and the publication of this volume. Two of the seven authors, Gunther Grimm and Rudolf Vierhaus, provide excellent essays that deal with a broadly conceived pattern of change in intellectual self-definition and social situations. Grimm traces the changes during the early modern period in the social position, role, and status of the German academic. These changes were accompanied by shifts in social function and academic tasks. Vierhaus, doyen of German Enlightenment historians, provides a brief, insightful analysis of both the self-definition and position of writers in the Enlightenment. Sieglinde Graf and Günther Birtsch have contributed analyses of local Enlightenment societies. Graf studied the obscure, but interesting, *Churbaierischen landwirthschaftlichen Gesellschaft von Oetting-Burghausen*, and Birtsch examined the important *Mittwochsgesellschaft* of Berlin. Both studies clarify the composition and functioning of these typical Enlightenment institutions. Hans Erich Bödeker looks at Gotthold Lessing's correspondence. He seeks to reconstruct an important communication network and demonstrates the vital importance of letter writing as a medium of Enlightenment sensibility. Martin Bollacher undertakes the task of resurrecting the now forgotten theologian Wilhelm Abraham Teller. He succeeds in demonstrating the importance of the popular "theological" Enlightenment. The last, longest, and potentially most interesting article was written by Reinhart Meyer. He analyzes late eighteenth-century changes in cultural consciousness by concentrating on the theater. Though rich in material pertaining to performing practice, the relationship between theater space and meaning, the forms of popular theatrical culture, and forgotten critical discussions of the art, the essay is hampered by a theoretical construct that is a pastiche of the stereotypes of substantialized class conflict, cooption, and repression that have populated too many historical analyses intent on protesting the commercialization of contemporary culture. Despite its occasional flaws, however, this volume enhances our understanding of the complexities of the German Enlightenment.

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HAGEN SCHULZE, editor. *Nation-Building in Central Europe*. (German Historical Perspectives, number 3.) New York: Berg; distributed by St. Martin's, New York. 1987. Pp. 208. \$27.50.

This volume is part of a series published in England called "German Historical Perspectives," collections of essays by German historians in translation intended "to overcome the language barrier"—a sad confession that language studies also languish in Britain. In his foreword, the editor, Hagen Schulze, notes the continued prevalence of simplistic views of German history and believes their existence is reason enough to offer the present essays embodying the most recent research and interpretations.

In his own essay on revolution and the rise of German nationalism, Schulze argues that the unusual concept of German unification as "revolution from above" needs to be qualified. He contends that the revolution drew much of its strength from mass nationalism from below and that Bismarck succeeded because he was able and willing to cater to this populist sentiment. Unfortunately, Schulze does not explain here how nationalism exuded such enormous appeal for the German masses, nor does he pay sufficient attention to the persistent strength of antinationalism in Germany. Dieter Düding offers a partial explanation for the spread of nationalist sentiment in discussing German patriotic societies and the extension of their appeal from the middle classes to the masses.

Adolf Birke treats the important subject of German Catholics and unification. He believes political Catholicism arose as a protest movement against state interference in church affairs, and he offers the challenging theory that it was a precursor to democratization in Germany—that is, that in some respects political Catholicism appeared earlier and was more comprehensive than either liberalism or socialism. In discussing German liberalism in the same context, Alexander Schwan reiterates the familiar point that liberalism as such failed in Germany because German liberals chose nationalism over freedom. He warns that, in the contemporary movement for German reunification, freedom could once again be sacrificed to the priority of unity.

Hubert Kiesewetter's cogently argued essay on economic preconditions for German nation building discounts the importance of the *Zollverein* and the supposed identity of economic interests among the various German states. He emphasizes instead the role of railroads and believes the economic interdependencies resulting from their construction were more important in promoting political unity than any other economic or fiscal

matters. He takes issue with Marxists and economic determinists, however, in arguing that unification was not a historical inevitability either on political or economic grounds.

Two essays deal with foreign reactions to events in Germany. Günther Heydemann shows that Palmerston's reactions to the crises of 1848 differed according to British interests. Essentially he wanted to maintain the status quo in Germany and avoid having to intervene in German affairs. In theory he favored strengthening Prussia as a counterweight to France and Russia, but he opposed any Prussian takeover of Schleswig-Holstein and hoped Prussia would respect the sovereignty of the smaller German states. The question of how Prussia was to be strengthened if it observed these conditions obviously did not concern him. In treating France and German unification, Michael Stürmer asks whether war with France was inevitable. His answer, not very clearly expressed, suggests that Bismarck had no alternative but to wage war or risk the defeat of his "conservatism," whatever that might have been.

Using the example of the revolutions of 1848, Harm-Hinrich Brandt shows that the mixture of nationalities in East Central Europe was so complex that any attempt at nation building had to be at the expense of other nationalities. An interesting counterpart to his argument is Klaus Zernack's view of Prussian and Polish nation building. He maintains that Prussia was an instrument of imperial and integrative nationalism to which the Poles could only respond with an increasingly uncompromising nationalism of their own.

A second and final essay by Schulze on Europe and the German question in historical perspective provides a survey of the subject and concludes that the Federal Republic's ties to the West are a categorical imperative for its survival. The current task of West Germans should be to preserve their cultural ties with the East without loosening their political bonds to the West or abandoning their constitutional standards.

NORMAN RICH
Brown University

HAROLD MAH. *The End of Philosophy, the Origin of "Ideology": Karl Marx and the Crisis of the Young Hegelians*. Berkeley and Los Angeles: University of California Press. 1987. Pp. ix, 305.

How German thinking got "from Hegel to Marx" has—for good reason—concerned generations of scholars. We have been told that the transition from Hegel to Marx involves weighty intellectual

issues: the transformation of idealism into materialism, the translation of humanist theory into socialist practice, the secularization of Christian modes of eschatological thinking. In his study of Marx, Hegel, Arnold Ruge, and Bruno Bauer, Harold Mah seeks to explain changes in German thought as a transformation of philosophy into ideology.

Drawing on biographical, contextual, and textual sources, Mah offers a balanced account of how the "theory of ideology" resulted from the "collapse of Hegelian philosophy" (p. 218)—overall, a solid piece of historical work. True, much is derivative, but Mah generously acknowledges his sources. Thus, the book serves as an excellent introduction to modern scholarship on Hegel and Marx. Specialists will find the chapters on Ruge—without doubt, a key player—most stimulating. Those chapters alone make an important contribution to German intellectual historiography.

It is impossible in a short review to discuss Mah's work in relation to other studies on the subject. Yet space permits a brief examination of the book's organization—around the theory of ideology—and the two problems that result.

First, Mah's concern is the "origin of 'ideology,'" and Marx is not the best focus for such a study. Indeed, Mah concedes that Marx often arrived a little late. He came to Hegelianism in 1837 when it was already dissolving and to ideology several years after Ruge. And Engels, Moses Hess, and Cieszkowski had all, before Ruge, seized on ideology as a factor in modern thinking in the context of detailed discussions about political economy, a topic Mah does not examine (p. 270 n. 1). Clearly, to approach Marx's discussion of ideology by way of biography restricts what Mah can say about Hess, Cieszkowski, and Engels and their role in developing the theory of ideology. So Mah is more or less silent about all three, which raises questions about the comprehensiveness of the study on its own chronological terms.

Second, Mah aims "to elucidate the intellectual and historical origins of [the theory of ideology as an] extremely cynical and highly influential way of thinking about abstract thought" (p. 2). But consider the following. Mah claims that in the 1820s Hegel's philosophy stood to Prussian politics as thought stood to action, yet, obviously, Hegel's thought included activism. And, according to Mah, between 1837 and 1841 changes in the disposition of the Prussian state precipitated "division within the Hegelian school" (p. 174). As a result, the "harmony of Hegelian philosophy and the Prussian state fell into jeopardy" (p. 39). Mah refers to this crisis among the young Hegelians as the antiphenomenological moment in the development of Hegelian philosophy. At that moment,

Mah declares, the young Hegelians began to regard Hegelian philosophy as "inverted," as an abstract force separate from the world rather than as an active force in it. Thus, they saw their task as reuniting philosophy with the world through their own program of action.

Here is a decisive point in the transition Mah describes. He appreciates the gravity of the issue, asking how Marx explained Hegel's "mystification of reality." Marx showed, Mah argues, Hegelian theory as, essentially, "conservative" (p. 188). Or, in other words, Marx recognized "the social limit of [Hegel's] abstract thought" and moved to overcome it (p. 42).

This is an incomplete explanation of what Marx did. To Marx, the purely theoretical character of Hegel's thinking made him conservative. What then enabled Marx to characterize Hegelian philosophy as abstract and theoretical? Much in Hegel's writings testified to his hostility to, and criticism of, theoretical abstraction. Moreover, the young Hegelians often cited Hegel's objectivity and realism as a remedy for romantic escapism; a down-to-earth quality made Hegel's thought so attractive in the 1820s. How, then, did this perception become reversed in a few years?

The answer, again, lies in the work of Hess, Cieszkowski, and Engels. Each depicted Hegelian philosophy as a system of abstraction. For each, moreover, the key was Lutheranism and Hegelianism as religious and philosophical forms of Protestant inwardness. Hegel had been critical of Protestant inwardness from the 1790s on. Thus, no means existed within the Hegelian tradition for presenting Hegel as the Luther of philosophy. One did exist, however, in the French intellectual tradition, in the writings of Saint-Simon and Auguste Comte. And Hess, Cieszkowski, and Engels drew heavily on that tradition between 1837 and 1843. Therefore, Hegelianism had to be assimilated to a French mode of thinking, itself already ideologically self-conscious, before being cast as ideology.

On Mah's own terms, then, to get from Hegel to Marx is difficult without first going through France—where the "hegemony of sensuousness" began and where idealism was first pronounced as simply theoretical metaphysics. In short, there is a "French connection" to the larger story of the origins of ideology between Hegel and Marx. Mah has done an excellent job on the German material. Obviously, more remains to be done.

LAURENCE DICKEY
Columbia University

SANDER L. GILMAN, editor. *Conversations with Nietzsche: A Life in the Words of His Contemporaries*.

Translated by DAVID J. PARENT. New York: Oxford University Press. 1987. Pp. xxvi, 276. \$26.00.

A surfeit of monumentalizing and debunking treatments of Nietzsche makes this volume's invitation to an antiquarian approach seem enticing indeed. One can accompany graduate students of the 1870s as they drop in on the ailing professor of classical philology, join the *artistes* of the eighties as they run into the amiable philosopher on his constitutional, or line up with the curious of the nineties as they are ushered into the sick room of the Great Man. To be sure, most of the reminiscences have been honed and polished over the years as the gulf between Nietzsche's obscure career and his posthumous fame has grown ever wider. Yet, even as "the many, all too many," relish and embellish their interactions with "the voice and the peak," they serve today to put Nietzsche back into a human context.

But do these snatches of a life add up to "conversations" comparable to Johann Peter Eckermann's conversations with Goethe, as Sander L. Gilman wishes to suggest? The Eckermanns and the Boswells permit the reader to take stock of both the subject and the reporter, however fictionalized the reportage. But the some seventy memorialists in this volume make Nietzsche appear as in a kaleidoscope. The cacophony of fleeting, forgotten voices does succeed, however, in submerging Elisabeth Nietzsche-Förster's dubious recall and questionable pronouncements. The compilation is fittingly dedicated to the late Massimo Montinari, whose edition of Nietzsche's books, letters, fragments, and notebooks has effectively dissolved all of the questionable editorial constructs of the past century, most notably the so-called *Will to Power*.

Approximately one-third the length of Gilman's German edition, *Begegnungen mit Nietzsche* (1981), this selection of a selection reduces the passages of Nietzsche's sister from 170 to 7, thins the accounts of Nietzsche's Basel colleagues and pupils, and dispenses with much of the "conversation" with the insane Nietzsche. Only a few cuts hurt, for instance, Levin Schücking's daughter recalling her afternoon in Rome with Nietzsche. Both the German and the English versions remind us that interiors have outer surfaces, that deep meanings coexist with everyday realities, and, when read in conjunction with letters to and from Nietzsche, these fragments allow us a glimpse of Nietzsche amidst the furniture of his time.

What sort of life emerges from this reading? A richer life, I think, than his biographers have been willing to grant: a lonely man making other lonely people, particularly intellectual women on their summer retreats, less lonely; a pioneering tourist of the new railway age repeatedly on the move; an

intellectual with his ear to the ground and his nose in the Parisian press. Just as his notebooks disclose how he worked and reworked his flowing, seemingly spontaneous prose, these reminiscences illustrate how he struggled against illness for a normality that could accompany his originality. Since hero worshippers and then fascists depopulated the Zarathustra landscape, one can only be grateful to Gilman and his capable translator, David J. Parent, for repopulating that world again.

PETER BERGMANN

University of Connecticut

MARIE-LISE WEBER. *Ludwig Bamberger: Ideologie statt Realpolitik*. (Frankfurter Historische Abhandlungen, number 28.) Stuttgart: Franz Steiner. 1987. Pp. 310. DM 88.

Ludwig Bamberger's leadership within the German liberal camp, his involvement in the major issues of the Bismarckian era, and his extensive published works and unpublished papers make him a fruitful subject for historical investigation. Marie-Lise Weber has produced a solid study of Bamberger's political activities and philosophical beliefs and incorporated them into the context of the German political scene. Although she has not uncovered any new unpublished material, she has integrated into her study the many significant secondary works that have appeared since 1974. She discusses Bamberger's development from revolutionary to "Realpolitiker," his contributions to the financial foundation of the German empire, his controversial roles in the *Kulturkampf* and the passage of the Antisocialist Law, the break with Bismarck over tariff issues, and the opposition to the chancellor's social program and colonial policy. She also analyzes the twists and turns in liberal politics as well as in Bamberger's relationship with the workers' movement.

This is not a study sympathetic to Bamberger. Focusing on his inadequacies, Weber's interpretation shifts the burden for supposed liberal failures from Bismarck to the liberals of the left, especially Bamberger. Weber chides him for losing his elasticity and openness and becoming rigid and closed to new developments so that ideological constructs displaced realistic considerations. She suggests that Bamberger did less than he could have from 1879 on to salvage gains from Bismarck's initiatives. Bamberger's ideologically motivated stance rather than Bismarck's evil genius was to blame for the liberals' failure and, by implication, for some of Germany's later problems. Bamberger's emphasis on the priority of economic freedom, Weber claims, alienated politically active entrepreneurs from liberalism. His unbending adherence

to Manchesterism undermined chances for a great liberal party in 1880. And his preoccupation with financial stability made him less than sympathetic to workers' needs. Bamberger might have been flattered to be considered so important.

Although Weber's argument represents a new or a revival of an old, almost National Liberal, approach to the Bismarckian period, it is not necessarily convincing. Issues such as tariffs, social policy, and colonialism, Weber claims, show Bamberger as dogmatically closed to new tendencies. She explains this, less than convincingly, by highlighting the influence of Hegel on Bamberger and by maintaining that Bamberger was more rooted in the preindustrial world because of his involvement in mining ventures in the 1850s and 1860s. Of course, Bamberger always ridiculed Arnold Ruge for his Hegelianism. And why did not Bamberger's later participation in the Deutsche Bank and his investment activity lead to an alteration of his views? Moreover, his friends attested to his openness to new trends in the arts. Why not in other areas?

Even if Bamberger had been more accommodating on certain issues, it is not clear how going along with Bismarck would have advanced the cause of liberalism. Weber ignores the nature of the German constitutional system in which Bamberger had to operate. One could demonstrate more flexibility if one controlled the system, as Bismarck did, or even if one thought it was evolving in a progressive direction, as Bamberger assumed until the mid 1870s. The concept of progressive versus retrogressive compromise might not fit in with the new realism, but it may still provide a better explanatory model for leftist liberal activity. Where would compromise lead? Should one accuse Bamberger of rigidity when Bismarck also wanted a Prussian economic council and two-year budgets that would undermine an already weak Reichstag? All this is to say that Weber, in her excellently researched work, makes Bamberger carry a heavy load for German history.

STANLEY ZUCKER

Southern Illinois University

FRIEDRICH HARTMANNSGRUBER, *Die Bayerische Patriotenpartei 1868–1887*. (Schriftenreihe zur bayerischen Landesgeschichte, number 82.) Munich: C. H. Beck, 1986. Pp. 1, 474.

This work, a slightly revised dissertation from the University of Regensburg, is impressive in several ways. First, it shows in exemplary fashion the thoroughness, insightfulness, and readability of some of the recent work done at German universities. Secondly, it demonstrates an awareness in

Germany of important research by Americans. Friedrich Hartmannsgruber gives appropriate credit to and makes good use of the efforts of American specialists, such as Frank Deedmyer Wright, Jr. ("The Bavarian Patriotic Party, 1868–1871," doctoral thesis, University of Illinois [1975]) and Gilbert Edwin Southern, Jr. ("The Bavarian Kulturkampf: A Chapter in Government, Church, and Society in the Early Bismarckreich," doctoral thesis, University of Massachusetts [1977]). Finally, and most important, in an age of rising American religious-political populism, this study of similar developments in late nineteenth-century Bavaria reminds us of both the short-term benefits and the long-term dangers for democracy of such movements.

Hartmannsgruber's subject is the little-studied, often-misunderstood, but remarkably popular Bavarian Patriotic party. He focuses on the period between its founding as a strictly Bavarian party in 1868 and its closer alignment with the national Center party, and its renaming as the Bavarian Center party, in 1887. His goal of analyzing the conditions and manner of the party's emergence, its self-image, and the social, regional, and confessional characteristics of its voters and representatives is an overwhelming one but one that he attains with confidence and sympathy. His sources—official administrative reports of the ministry of the interior, party newspapers, the correspondence of party members, and a wealth of secondary works—are reassuring to the critic and very helpful to the student of the period.

Hartmannsgruber's conclusion, after an exhaustive survey of party activity in the Bavarian diet and the imperial Reichstag, is that the Bavarian Patriotic party is difficult to pigeonhole. In its sociological structure it embodied those elements of society—peasantry, artisans, landowners, clergy, and nobility—that resisted the forces of change represented by liberalism, statism, and modern militarism. It was, however, also a party that resisted the popular anti-Semitism of late nineteenth-century Germany. Confined by its allegiance to a constitution and to a king, both institutions adroitly used by liberal factions to thwart its resistance to modernity, the party remained fragmented and checkmated despite its status as the strongest party in the Bavarian diet. Yet, by contributing to the political awakening of the Bavarian population, even if in the interest of conservatism, the Bavarian Patriotic party prepared the way for the emergence of modern democracy. Hartmannsgruber grants, however, that, unfortunately, it was only much later, after the sad experiences of the Weimar republic, that political Catholicism in Germany learned to pro-

tect itself and democracy from the dangers of an unrestrained populism.

Hartmannsgruber's study is most worthy of careful and sympathetic reading. His story of the frustrations of conservative Catholics forced to use the methods of democracy in order to defend their religious, social, and federalist beliefs against the onslaught of liberalism, capitalism, and nationalism (in a Prussian militaristic mold) is poignantly and thoughtfully told. It is an excellent counter to the remnants of the Prussian school of liberal and nationalist historiography.

ROBERT D. BILLINGER, JR.
Wingate College

HANS-GEORG ASCHOFF. *Welfische Bewegung und politischer Katholizismus 1866–1918: Die Deutschhannoversche Partei und das Zentrum in der Provinz Hannover während des Kaiserreiches*. (Beiträge zur Geschichte des Parlamentarismus und der Politischen Parteien, number 83.) Düsseldorf: Droste. 1987. Pp. 433. DM 78.

When the Kingdom of Hanover was annexed to Prussia during the rapid succession of events leading to the foundation of the German Reich, a significant segment of the state's population remained unreconciled to the change and continued to protest the illegality of the annexation. These "Guelphs," taking their name from the Hanoverian *Welf* or Guelph dynasty, hoped to restore the dynasty and establish Hanover as an autonomous state in the Reich. For over twenty years, the German Hanoverian Party (DHP) was a major political force in the new Prussian province of Hanover, dividing its Reichstag votes almost evenly with the National Liberals. After 1890, the party lost much of its strength at the polls and dwindled to the rank of a splinter party. Among the reasons for this, apart from the obvious effect of the passage of time, were the growth of Social Democracy and other political competition in Hanover and the distancing from the Guelph movement of the dynasty itself. When in 1913 the sole surviving heir to the pretender married the daughter of William II, made his peace with Prussia, and was permitted to succeed to the vacant throne of the state of Brunswick, the party lost much of its reason for being.

The Guelphs benefited from their close alliance with the Catholic Center Party, and the two parties agreed on a similar platform: opposition to Bismarck's methods; defense of state and regional autonomy and of the rights of the individual; preservation of confessional schools and freedom for the churches (the Guelphs were successful in preventing the merger of the Hanover state

church with the Prussian Evangelical Union); and opposition to excessive military expenditure. Both parties fought the *Kulturkampf* legislation, the Socialist Law, and other "exceptional" laws. The Hanoverian loyalties of Ludwig Windthorst and his close friendship with the Guelph leader Ludwig Brüel fostered the alliance and the Center Party valued it as proof of its claim to be interconfessional. After Windthorst's death, the ties became less close, in part because, in Guelph opinion, the Center deserted the opposition to become a government supporter.

Those readers who want to learn every possible detail about the activities of the German Hanoverian Party in all nineteen electoral districts of the province will find this latest volume from the Commission for the History of Parliamentarianism and Political Parties wholly satisfactory. Those who would like to consider the larger implications of the persistence of the Guelph movement (which continued through the Weimar period and resurfaced briefly in 1945), and how it relates to other forms of regionalism, will find themselves frustrated, because the author draws few conclusions from his study. His opening pages introduce some interesting ideas, but the book does not follow through on them, and his brief "conclusion" consists mostly of a sketch of the DHP's Weimar years. Hans-Georg Aschoff's most provocative remark is his last sentence: "In the creation of the *Land* of Lower Saxony . . . [after World War II] an important demand of the old German Hanoverian Party was realized" (p. 338).

ELLEN L. EVANS
Georgia State University

HENRICH POTTHOFF. *Freie Gewerkschaften 1918–1933: Der Allgemeine Deutsche Gewerkschaftsbund in der Weimarer Republik*. (Beiträge zur Geschichte des Parlamentarismus und der politischen Parteien, number 82.) Düsseldorf: Droste. 1987. Pp. 435.

The decline of Germany's socialist labor movement from a position of decisive importance in 1919–20 to one of utter impotence in 1932–33 constituted a critical chapter in the collapse of the Weimar Republic. With nearly eight and one-half million members at the peak of its influence in the early 1920s, the socialist labor movement represented one of the most formidable and effective pillars of Weimar democracy. Yet by the middle of 1932 the combined effect of parliamentary paralysis and economic crisis had severely impaired the movement's ability to resist the rising tide of right-wing radicalism that was in the process of sweeping the country. Not only were the leaders of the socialist labor unions powerless to prevent

Hitler's appointment to the chancellorship in January 1933, but they were unable to save the movement to which they had dedicated their lives from destruction at the hands of the Nazi state four months later.

What were the specific factors that help explain the tragic decline of the socialist labor movement between 1919 and 1933? It is to this question that Heinrich Potthoff turns in his judicious and carefully reconstructed history of the General German Trade-Union Federation (*Allgemeiner Deutscher Gewerkschaftsbund* [ADGB]), the umbrella organization of the socialist labor movement from 1918 to 1933. In doing this, Potthoff provides a wealth of information about the ADGB's organizational and membership structure, its accomplishments on behalf of the German working class, its proposals for a transformation of the economy and society, the political environment in which it had to operate, and the role of unions in the Weimar political system. In explaining the ADGB's passivity in the last years of the Weimar Republic, Potthoff cites not only the demoralizing effects of the deepening economic crisis but also the fact that the ADGB's commitment to the republican form of government, particularly as it was embodied in the Weimar Republic, had been severely compromised by its inability to secure adequately the interests of the German working class within the framework of that system.

Ultimately, however, Potthoff's study is both more and less than what it purports to be. For, although Potthoff has taken great pains to embed his history of the ADGB in a rich mosaic of detail about the social, economic, and political history of the German working class and has done an excellent job of establishing the constraints under which the leaders of the ADGB, both as defenders of Weimar democracy and as representatives of organized labor, were forced to operate, the author has been less successful in abstracting the history of the ADGB from the general background in which that history is rooted. By the same token, Potthoff's study is to be appreciated not so much for the new insights it brings to bear on labor's plight in Weimar Germany as for the strength and sweep of its synthetic vision. In this respect Potthoff's study is a truly admirable accomplishment, and it will remain the definitive history of the ADGB for some time to come.

LARRY EUGENE JONES
Canisius College

ULRICH VON HEHL, *Wilhelm Marx 1863–1946: Eine politische Biographie*. (Veröffentlichungen der Kommission für Zeitgeschichte, number 47.) Mainz: Matthias Grünewald. Pp. xlv, 503. DM 64.

Those familiar with Weimar Germany's political history will remember that Center party leader Wilhelm Marx (1863–1946) was an extraordinarily active politician. He not only chaired the Center party and its Reichstag delegation between 1921 and 1928 but also headed four coalition governments between 1923 and 1928. In addition, he held office briefly as minister president of Prussia, ran unsuccessfully for president against Paul von Hindenburg in 1925, and served as minister of justice in Chancellor Hans Luther's second cabinet in 1926. In addition, Marx early on was involved in the *Volksverein für das Katholische Deutschland*, the Catholic school organization, the Prussian Landtag, and the Reichstag. One of the strengths of Ulrich von Hehl's carefully crafted political biography is his detailed attention to Marx's early political career. Hehl traces the steps that led this devout Catholic from a promising career in the Prussian judiciary to active involvement in Catholic social organizations and a leading role in Düsseldorf's Center party. The author also explains how Marx's basic political orientation evolved from a religiously based sense of duty, commitment to equal rights, and opposition to socialism and secularization.

Equally revealing is the discussion of how this relatively unknown Rhinelander rose to national political prominence after World War I. Part of the explanation lies in Marx's Rhineland political base, important party connections, traditional centrist principles, personal integrity, and considerable skill in mediation and negotiation. Another part of the explanation lies in the Center party's bitter factionalism and the republic's parliamentary stalemate after 1920, both highly unstable situations calling for leaders of compromise and accommodation like Marx.

Yet few politicians appeared less suited for national leadership. Marx opposed the republic at the outset and continued to harbor doubts for some time about the ability of parliamentary democracy to maintain public order and to promote public well-being. Later, as chancellor, he faced economic and diplomatic issues for which he was largely unprepared by either training or experience. Finally, during the presidential contest in 1925, Marx seemed uncomfortable with the techniques of modern political campaigning.

Unfortunately, von Hehl's narrow political approach makes it difficult to estimate the extent to which Marx's religious opinions, personal life, and political style influenced his political behavior and outlook. The focus is so tightly fixed on formal political activity that there is little opportunity to explore Marx's mature political values or the character of his public leadership.

All the same, the author presents the first well-

researched political profile of a tireless crisis manager who contributed unselfishly to the fragile political stability of the Weimar Republic. In so doing, von Hehl helps us understand why contemporaries never ranked Marx among the great leaders of the day and why the Center party produced so few outstanding leaders for such troubled times.

RENNIE W. BRANTZ
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GÜNTHER KÖNKE. *Organisierter Kapitalismus, Sozialdemokratie und Staat: Eine Studie zur Ideologie der sozialdemokratischen Arbeiterbewegung in der Weimarer Republik (1924–1932)*. (Studien zur modernen Geschichte, number 37.) Stuttgart: Franz Steiner. 1987. Pp. xii, 388. DM 88.

Günther Könke's study of Social Democratic ideology in the second half of the Weimar era offers a valuable new synthesis on a much-debated theme. Focusing on the analysis of "organized capitalism" and the introduction of *Wirtschaftsdemokratie* as the socialist goal, Könke pursues an "examination of ideological 'attitudes' [*Einstellungen*], which are defined in the sphere of society and economy and lie at the root of political action" (p. ix). This is not as exceptional a methodology as the author implies, but he employs it most effectively. The result is less new information—most of the sources are published—than a deeper comprehension of the historical context. Although the density of his prose tends to match the solidity of his content, Könke advances our understanding of the orientation of Weimar Social Democracy.

"Organisierter Kapitalismus" was an optimistic concept. Emphasizing structural change, it portrayed capitalism as having developed to the point where, in spite of itself, it was setting the foundations for a socialist economy. Some of the signposts of this hypothesized progress—the extension of state involvement in economic affairs during World War I, the phenomenon of rationalization in American industry—are familiar in the literature. Könke gives them their due but in addition demonstrates how much both the existence of the republican state and the organizational mode of Social Democratic thinking and action disposed authorities like Rudolf Hilferding and Fritz Naphthali to hold to the institutional bias of their guiding analysis. The goal of *Wirtschaftsdemokratie*, which they elaborated in the late 1920s, fit comfortably into this context and sanctioned the placid reformism of the Social Democratic party (SPD) and unions as correct policy leading to the inevitable socialist future.

All the same, as Könke convincingly demon-

strates, *Wirtschaftsdemokratie* was a flimsy ideological garment even in the favorable climate of Weimar's stable years. Analytically, it put too positive a value on industry's trend toward cartelization by ignoring or playing down its reactionary political implications. Similarly, Social Democratic theorists optimistically misread the impact of state intervention both in its control over the economy and in its potential to represent the common interest rather than to adjust to the competitive norms of the business world. Meanwhile, the economic prognosis of the Social Democrats remained superficial.

It comes as no surprise, then, that the SPD and unions were at a theoretical loss in 1929–30, when the depression struck and right-wing elements substituted presidential government for the parliamentary regime. Könke shows, however, that the critics within their ranks possessed little more wisdom. In most respects the left opposition shared the perspectives of the ideological leadership. More credit is given to the formulators of the WTB plan of 1932, not because it was an economic cure-all but on the ground that it could be a trigger for activist policy. Yet already with the unions' program *Umbau der Wirtschaft* of November 1932 the effort to escape a passive ideology was over. In the larger scope Könke argues that Weimar Social Democracy chose a different posture than Karl Marx would have assumed. Here he is unpersuasive, an exception in this very well articulated analysis.

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Albany*

ERNST TOPITSCH. *Stalin's War: A Radical New Theory of the Origins of the Second World War*. Translated by A. TAYLOR and B. TAYLOR. New York: St. Martin's. 1987. Pp. 152. \$19.95.

This brief book, originally published in German in 1985, contains the ruminations of a professor of philosophy at Graz University on the origins and nature of World War II. We must learn here that, in the good professor's view, most of the old fairy tales are true after all. Hitler really did want peace and was thwarted in this laudable aim first by the British, who encouraged Poland to resist fair German demands, then by the British and French when they refused to make peace after the defeat of Poland, and thereafter by the Soviet Union, which decided in the summer of 1940 to attack Germany, then deliberately provoked Germany during the visit of Foreign Commissar Vyacheslav Molotov to Berlin in November 1940 (so that the Soviet Union could appear as the innocent victim), and finally planned to attack Germany itself in

1941. Not only did the Soviets provoke the poor peace-loving Germans into attacking them, they were also responsible for the cruelty characteristic of the war in the East.

When the more elaborate ruminations cannot be based even on a distortion of the evidence, the author simply uses his imagination. The plans for the Soviet attack in 1941 are described in considerable detail (pp. 110–11) but with no evidence cited for them. Where the evidence shows that the memoir literature is faulty, as for example in the quotation attributed to Hermann Göring for September 3, 1939, at a meeting he did not attend (p. 39), Ernst Topitsch prefers to rely on a fake example that suits his theory. Historians will be interested to learn here that the German ambassador to France was murdered in 1938, that Britain and France took part in the Soviet-Finnish War of 1939–1940, and numerous other novel fictions.

Since the author believes that “most German documents relating to the war were published soon after the war ended” (pp. 4–5), when in fact at that time the overwhelming majority of such documents were still closed, he has not found it necessary to do any research in either German or other archives. The only thing radically new about this book is a reputable publisher printing such trash.

GERHARD L. WEINBERG
University of North Carolina

HEINZ HÜRTE. *Verfolgung, Widerstand und Zeugnis: Kirche im Nationalsozialismus: Fragen eines Historikers*. Mainz: Matthias Grünewald. 1987. Pp. 130. DM 19.80.

The church struggle, it need hardly be said, has become one of the hardy perennials of modern German historiography. In this small volume, Heinz Hürten offers a mosaic of reflections, rooted in a comprehensive reading of recent scholarship, on the problem of defining and evaluating ecclesiastical resistance to Nazism, chiefly that of German Catholicism. Numerous brief but thematically linked sections—nearly fifty subheadings for just over a hundred pages of text—attempt to identify the central “zone of conflict” (p. 20) between church and regime, to explore the multiple and sometimes contradictory levels of meaning in terms such as resistance, and to suggest a possible typology of criteria by which to evaluate the church’s institutional and theological stance after 1933.

Hürten’s discussion is thoughtful, judicious, and low key. Writing primarily for nonspecialists, he is

less concerned with unearthing new evidence or advancing new interpretations than he is with underscoring the need for caution and a due regard for the complexities of historical experience. In the end, he argues, the behavior of the church eludes conclusive historical judgment. The book’s cover, fittingly enough, is a subdued study in halftones.

This is not to say that Hürten lacks a distinct point of view. One of his book’s virtues, in fact, is its transparent sensitivity to the values and presuppositions that inform historical analysis. In Hürten’s case these values are clearly those of a loyal Catholic who regards the Third Reich as a totalitarian negation of everything the church claims to be and profess. For this reason he wants to insist—and this constitutes his challenge to fellow scholars—that the governing factor in assessing the church’s actions after 1933 cannot be the scope of its (political) resistance but rather the integrity of its (spiritual) witness in the face of systematic neopagan persecution. Hence, the title of his book. For historians qua historians to sit in judgment over the church’s motives and purposes would be to exceed the bounds of their professional competence. Historical research may help establish facts and suggest insights, but it cannot pretend to provide a sufficient basis for final judgments in matters ecclesiastical. These are properly the province of theology. Only the church itself, in short, has ultimate authority to judge its actions.

Not all scholars are likely to find such arguments compelling. Specifically Catholic assumptions aside, Hürten’s notions about the task of history will strike many historians (including, I suspect, many who are practicing church members) as both overly tidy and strangely circumscribed. His insistence on a totalitarian model for understanding the Nazi regime also seems a bit simplistic—and a bit surprising, because Hürten’s deft review of recent work on everyday life in the Third Reich would lead one to expect similar attention to the ongoing debates over the actual workings of the Hitler state. Perhaps such omissions are part of the price of Hürten’s treatment, which is concise and deliberately selective throughout.

Casual perusal might suggest that Hürten is an uncritical apologist for the Vatican and the German bishops. That would be a pity, for such is hardly the case, and on the whole Hürten’s musings are marked by considerable learning and good sense. If he fails to arrive at clear-cut answers to many of the questions he poses, this failure only confirms the obvious—neither the passage of time nor the writing of much history has managed

to diminish the Third Reich's continuing power to disconcert and perplex.

DAVID J. DIEPHOUSE
Calvin College

CLAUDE J. LETULLE. *Nightmare Memoir: Four Years as a Prisoner of the Nazis*. Foreword by AMOS PERLMUTTER. Baton Rouge: Louisiana State University Press. 1987. Pp. x, 132. \$16.95.

Evaluating personal descriptions of horror decades after the described events provides a daunting challenge for any reviewer. On the one hand is the difficult professional issue of time's passage, of uncertain memory, of source credibility, of human emotion. On the other hand is the dreadful, known reality of human savagery, personal mistreatment, group suffering, and national shame.

The volume provides an example of this professional difficulty. Claude J. Letulle, a French prisoner of war in Germany from 1940 to 1944, describes his horrific experiences in riveting detail. He suffered a wide range of indignities and abuses as a farm worker, laboratory assistant, and general slave. His descriptions are clear and pointed in explicating the depravity of war through an individual's activities. His book is neatly written and nicely organized. One reads it with profit.

At the same time, one marvels at the author's memory after so many years. Specific details, precise numbers, remembered conversations enhance the story's clarity but also raise concerns about the strength of memory. Under the circumstances, one should be careful about demeaning a vivid story of suffering and pain.

CHARLES B. BURDICK
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Paolo Giovio: Il Rinascimento e la memoria. (Proceedings of the Raccolta Storica Conference, 1983; Pubblicata dalla Società Storica Comense, number 17.) Como: La Società a Villa Gallia. 1985. Pp. 330.

Paolo Giovio is an intriguing historical figure. His life spanned the turbulent years of the Italian wars. He was a bishop, confidant to popes and rulers (notably the Medici and Farnesi), patron and collector of the arts, and author of sophisticated historical works. In 1983, on the five hundredth anniversary of his birth, a conference was convened in his native Como. The volume under review is the collection of papers from that conference. These papers indicate why Giovio is an

object of study, and a few of them go a long way toward demonstrating his historical importance.

For whatever reasons a number of English-speaking scholars have been drawn to study Giovio. Those interested in his historical works have written the most provocative essays in the volume. Most dedicated to a study of Giovio has been T. C. Price Zimmermann, whose paper on Giovio and the crisis of the Cinquecento led off the conference. Zimmermann argues that Giovio, a proponent of the stability of princely as opposed to republican government, was most profound about politics in his historical writings. The late Eric Cochrane's paper traces the equivocal reputation Giovio has enjoyed at the hands of scholars who have disliked his monarchist leanings. But, as Cochrane points out, Giovio, unlike his more famous contemporaries Niccolò Machiavelli and Francesco Guicciardini, was a "professional" historian who produced not a Livian moralizing history of political and military doings but a commentary on causally connected events. Yves Giraud's paper on Fortune in Giovio's works is a nice complement to Zimmermann's and Cochrane's papers, as he delineates Giovio as a realist whose view of human possibilities in the face of Fortune placed him between Guicciardini's pessimism and Machiavelli's relative optimism. Michele Cautadella's analysis of Giovio's *Vitae* relates these biographical sketches to his historical works.

For the rest, the papers dealing with other aspects of Giovio seem intent only on displaying the author's erudition. Pietro Gini, president of the Società Storica Comense, sponsor of the conference, discusses Giovio, for example, in relation to sixteenth-century religious life mainly by telling where Giovio was at various moments. Surely someone so well versed in his subject could have offered more. In a similar vein there are papers with less than inspiring accounts of Giovio's interest in Ethiopia, his impact on art in Hungary, his collection of Flemish tapestries, and so forth. They consistently fail to relate Giovio's thoughts or activities to wider historical or art historical problems. Worthy of note is Julian Kliemann's account of Giovio's influence on art through the formulation of *invenzioni* for the Medici villa at Poggio a Caiano and the Sala dei Cento Giorni painted by Giorgio Vasari for Cardinal Alessandro Farnese in Rome; on the Sala dei Cento Giorni, the paper by Clare Robertson is also illuminating. Here at least the problem of the impacts of patron and artist on the work of art is raised in an interesting fashion. The only survey of Giovio's influence comes in Ernesto Travi's concluding review of the conference's papers. His essay, however, is a paper of celebration and not a critical vehicle, so there is no

summation of the important questions and future directions of Gioivo studies.

THOMAS KUEHN
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PAOLO PISSAVINO. *Lodovico Zuccolo: Dall'audizione a corte alla politica*. (Pubblicazioni della Facoltà di Lettere e Filosofia dell'Università di Pavia, number 36.) Florence: Nuova Italia. 1984. Pp. 138. L. 14,000.

In a brief monograph, Paolo Pissavino attempts to provide the reader with an intellectual biography of Lodovico Zuccolo, a rather brooding philosopher and political theorist whose ambitions and experiences at the court of Francesco Maria II, duke of Urbino, brought little joy to dispel the gloom that often surrounded him. Zuccolo was born at Faenza on September 18, 1568, and taught at the Università delle Arti of Bologna in 1607–08. In 1608, he was called to the service of Duke Francesco Maria II and remained in Urbino until 1617, when his unhappy sojourn was ended. After appointments at the university, first at Bologna and later at Padua, he remained in Spain in the service of the nuncio Monsignor Innocenzo Masimi from 1623 to 1625. He died in 1630 in Bologna, a sad and unfulfilled scholar who wrote in his *Considerationi* that “non ho mai trovato un porto, nè spiaggia” (“I have not ever found a port nor a beach”).

Part of Zuccolo's sadness may have been the result of the suffering of his father, the noble Alessandro, who died on a galley while serving a sentence for heretical opinions. Lodovico himself was accused before the Holy Office of the Roman Congregation for denying the immortality of the soul and for referring to Epicurus, Galen, and Fra Paolo Sarpi.

Past studies on Zuccolo have portrayed him as a theoretician of statecraft, as a utopian thinker, and as a free thinker, the last according to the interpretation of Carlo Ginsburg. Pissavino emphasizes Zuccolo's contributions to the history and philosophy of science but sees him especially in the role of political critic. Zuccolo's scientific concerns, as expressed in his *De quantitate solis disceptatio*, may have been quickened by his desire to have a post at the court of Duke Francesco Maria II, who maintained a lively interest in the sciences as well as the arts.

Zuccolo dedicated his *Heroica virtus sive de honesto gloriae studio* to the young prince, Federico Ubaldo, son of Francesco Maria II, whom he urged to pursue his education with diligence so as to keep the idea of heroic virtue before his eyes. Yet, in an interesting passage from *Oracolo LXXI*,

Zuccolo paraphrased Aristotle in discussing the difficulty of the pursuit of virtue at the court, when he wrote, “ut vel recte se habeat ad virtutem, vel semibonus quidam fit, et non malus sed semimalus” (“that he either direct himself rightly toward virtue or become a half-good man and not evil, but half-bad”). This statement seems to describe Zuccolo's opinion about his fellow courtiers (and perhaps himself) and was perhaps one of the causes for the unhappiness he experienced at Urbino. Zuccolo's best-known work was the *Considerationi*, in which we observe his cautious rapport with Aristotle.

Pissavino has written a brief introduction to the life and thought of Zuccolo that is filled with detailed information in the notes, which are actually longer than the text itself. His aims were modest, as he states in his preface, and he has presented the reader with informative bits of knowledge about scientific arguments in the early seventeenth century. One would like to know more about the ambience of the Urbino court and the true relationship of Zuccolo to Duke Francesco Maria II, but we can only surmise the answers to many interesting problems implicitly argued.

This monograph is valuable to the specialist in the history of science. It also whets an interest in Zuccolo, his writings, and his role at the court of Urbino, and one hopes that in a second volume a fuller discussion of many significant points will be presented.

MARION LEATHERS KUNTZ
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CLAUDIO G. SEGRE. *Italo Balbo: A Fascist Life*. Berkeley and Los Angeles: University of California Press. 1987. Pp. xvi, 466. \$29.95.

This is one of three major studies of Italo Balbo to appear within the last five years, the other two being Giordano Bruno Guerri's *Italo Balbo* (1984) and Giorgio Rochat's *Italo Balbo* (1986), both available only in Italian. In English, Claudio G. Segre's full-length biography contributes to an understanding of Balbo particularly sought by scholars interested in military history and readers of a certain age who may remember him as one of the more dashing and popular figures of Benito Mussolini's Italy.

As Segre's study makes clear, Balbo's activities were not confined to military matters, and his talents were not purely promotional. This well-researched biography brings out Balbo's important role in matters administrative, political, and social, demonstrating in the process that he was an effective organizer, a quality disguised in his own time by the reputation for glamour. His organiza-

tional skills were already evident in his early exploits as leader of the Fascist squads. In that capacity, Balbo distinguished himself for his ability to redirect the techniques of military combat against those "other Austrians" (p. 37), as he called the Socialist opponents of Fascism. Balbo thus exemplified, probably better than anyone else, the tendency to transfer the mentality of war to domestic political contests, making him largely responsible for an enduring legacy of fascism in the politics of our own times.

But this aspect of Balbo's work was already well documented, nowhere more thoroughly than in Paul Corner's *Fascism in Ferrara, 1915-1925* (1975). The message of Segrè's biography is more complex because he follows Balbo throughout his political itinerary, from his youthful days as a self-proclaimed Mazzinian radical to his fiery death in a plane crash, a few weeks after Italy's entry in World War II, the accidental victim of gunfire from his own antiaircraft batteries.

Like many other political activists of his generation, Balbo was marked for life by the experience of World War I in which he served as a volunteer and emerged as a decorated veteran. His subsequent career falls into three distinct phases. From about 1921 to 1925, he was the street fighter and political boss of Ferrara, which became his private political fief after he had rooted out the opposition and ingratiated himself with local landed interests. From 1926 to 1933, he was minister of aviation, a post that brought him world-wide recognition for his transatlantic group flights. From 1934 to 1940, he was the governor of Libya, responsible for the military defense and economic development of that colony. As colonial governor, he once again demonstrated his organizational skills and talent for publicity by settling some twenty thousand Italian agricultural emigrants in Libya. Segrè's earlier book, *Fourth Shore: The Italian Colonization of Libya* (1974), detailed this last phase of Balbo's career.

Segrè's close and balanced look reveals the strengths and weaknesses of Balbo's personality, his flair for publicity, his organizational skills, qualities of leadership, and uneasy relationship with Mussolini. Although Segrè's account is basically sympathetic, the final impression is unavoidably mixed, for Balbo combined dash with glaring character and political deficiencies. Repeatedly, at critical moments, he was not equal to the challenge, showed a lack of moral courage, or blinded himself to the significance of his actions. When Balbo's squads were burning down worker cooperatives, he insisted that he was only destroying the strongholds of Italy's enemies. When Mussolini established a suffocating personal dictatorship, he grumbled but acquiesced. He opposed the

alliance with Nazi Germany but welcomed and feted Nazi leaders in Libya. When racial policies were instituted in 1938, Balbo limited his protests to expressions of personal regard for his many Jewish friends. Segrè confronts these dilemmas and gives enough information for us to ponder the contradictions and shortcomings of this Fascist life.

ROLAND SARTI

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FLORIANA COLAO. *Il delitto politico tra Ottocento e Novecento: Da "delitto fittizio" a "nemico dello stato."* (Quaderni de "Studie Senesi," number 59). Milan: A. Giuffrè. 1986. Pp. ix, 407.

Whether continuity or change most characterized the transition from a liberal monarchy to a Fascist dictatorship in Italy has not been decided. This issue is becoming even more complex, as new research points toward an increasingly nuanced interpretation of political, economic, and social developments during the crucial years after World War I. Floriana Colao reaches back chronologically to the nineteenth century, but her history of political crime has the question of continuity or change at its center. Colao analyzes different legal theorists, the Zanardelli and Rocco penal codes of 1889 and 1930, and various decisions of the supreme *Corte di Cassazione* to support her contention that the evolution of legal doctrine has not been simple and progressive. Periods of repression of political dissent—fascism, for example—sometimes follow eras of toleration, although she warns against simplistic labels that ignore the mixture of tradition and revision typical of all schools of constitutional thought.

At the time of Italian unification, the "classical school," which traced its origins to Cesare Beccaria's *Dei delitti e delle pene* (1764), dominated legal discourse. Taking individual freedom of will as axiomatic, legal writers such as Francesco Carrara and Enrico Pessina recommended for common, nonpolitical offenses a clear scale of punishment calibrated to fit the crime. Yet, as liberals, they characterized political dissenters as noble conspirators against monarchical absolutism and recommended lighter penalties for them. In Carrara's and Pessina's view, a future regime of popular sovereignty would champion, not punish, these educated, bourgeois supporters of national unity and civil rights. Thus, liberal jurists opposed extradition of political prisoners, argued that juries rather than state-appointed judges should hear their cases, supported amnesties by the king, and recommended that prisons allow reading, writing, and conversation.

The publication of Cesare Lombroso's *L'uomo delinquente* in 1876 signaled the rise of a second theory of criminology, that of the "positivist school." Colao argues that, despite their verbal polemics with the classicists, positivists such as Lombroso and Enrico Ferri tended to preserve distinctions between common and political criminals, which favored the political prisoners. Both men denied that criminals had free will but reserved their label of "born criminal"—that is, one born with physical and psychological aberrations—for common offenders. In their view, which was influenced by socialism, the true political criminal—the revolutionary representing the masses and calling for progressive change—formed part of the healthy Darwinist evolution of society and therefore deserved minimum retribution. By the late nineteenth century, however, the successful establishment of a liberal Italian monarchy and the desire to protect its stability encouraged a partial reevaluation of dissenters. For example, both positivist criminologists and magistrates redefined anarchist violence as common crime in order to seek extradition of, and apply harsh punishment to, perpetrators.

By the advent of Mussolini's rule in 1922, two other legal trends competed with the classical and positivist theories: the "third school," which sought to take politics out of the law and substitute a neutral and technical approach to legal interpretation, and idealism, which stressed the moral and spiritual aspect of the social defense of the state. The Rocco Code retained some continuity with the past and respect for legality in criminal procedure, perhaps because it drew from all four schools. Yet Colao emphasizes the vast shift in the definition of political crime and political defendants by a regime that preached obedience by the individual to the state. Rather than a violation of the security of the state as defined in the Zanardelli Code, political crime was envisaged as violating the personality of the state, considered an ethical being by Fascists. As enemies of the state, political prisoners faced harsher conditions than common delinquents.

Colao's book lies squarely within the Italian tradition of sophisticated and learned intellectual history of law but lacks the more innovative thrust of modern social historians of crime who scrutinize the law's effects. She rarely touches on everyday interpretations of the penal code much less its practical consequences for political dissenters or citizens. How did lower court judges and police officers, who handled the bulk of criminal cases, understand and apply the political provisions of the penal code? Did the theoretical clash among legal schools significantly alter the policies or behavior of the large, bureaucratic ministries of

justice and interior? Although the narrowness of Colao's methodology excludes answers to these types of questions, she does broaden the geographical scope of her account by juxtaposing parallel legal developments in France and Germany. Her solid and detailed study will be of interest to political historians of modern Italy as well as historians of comparative law and crime.

MARY GIBSON

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P. G. M. DICKSON. *Finance and Government under Maria Theresa, 1740–1780*. Volume 1, *Society and Government*; volume 2, *Finance and Credit*. New York: Clarendon Press of Oxford University Press. 1987. Pp. xviii, 491; xv, 453. \$85.00; \$79.00.

These are two impressive volumes. The amount of research that must have been expended to write them boggles the mind. In addition to the text, there are extensive charts, tables, lists, and footnotes full of various kinds of information. In volume 1 the appendixes alone run to more than one hundred pages, and in volume 2 to almost eighty. No one working in eighteenth-century Austrian history will be able to ignore this work because the data include such diverse topics as charts of the size and location of the Austrian army for every year from 1695 to 1794, Lord Stormont's description of the monarchy in 1765, and an index of Viennese warehousemen and wholesalers from 1740 to 1780.

Although the study comes in two volumes, the organization is really in three parts. Part 1, which includes the first eight chapters of the first volume, discusses the social composition of the monarchy; part 2, which encompasses the rest of that volume, describes the structure and makeup of the government; and all of volume 2 presents finances. In the first part, that on society and social composition, P. G. M. Dickson makes clear one of his main themes: on the basis of the raw data, it is very difficult to draw general conclusions about the composition or resources of the various social strata of the monarchy. In fact, it is difficult even to be absolutely certain what these strata were. Only in a few places does he present solid conclusions. One is that the traditional view of the Bohemian nobility being replaced after 1620 is generally true, but the opinion that large amounts of Hungarian land were owned by foreigners or by Magyar families living primarily in Vienna is false. Another is that Maria Theresa set out at the beginning of her reign to reduce radically the dependence of the monarchy's income on Jewish financiers and did so by turning to the ranks of the

privileged warehousemen and wholesalers (some of whom were even Protestants), who had contacts with banking houses in Belgium and Genoa.

The section on government especially appealed to me because, working primarily on eighteenth-century Habsburg foreign policy, I had often wondered how the government actually functioned. My research routinely encountered domestic offices that seemed to appear out of nowhere and, after a year or two, to vanish without a trace. Likewise, an official who was president of an office a few documents ago seemed abruptly to become the head of another office, the existence of which I had not yet discovered. Dickson has tracked down many of the changes of both the offices and the leading personnel in them and, in doing so, clearly shows that there was an astonishing fluctuation—but general increase in size—of government offices between 1740 and 1780. As the government grew, so did the amount of paper generated by bureaucrats. The impression is that the eighteenth-century Austrian administration followed the pattern of most modern governments: it undertook reform in the interests of improved efficiency and neat patterns, which then broke down fairly quickly as human problems refused to adjust to the categories the government assigned to them, which in turn inspired the ministers to compose yet more reforms.

The volume on finances is truly impressive, if difficult to wade through. Dickson notes that the great reforms of Count Friedrich Wilhelm Haugwitz in 1748–49 came about not so much to get Silesia back but because of the fear that Frederick the Great's next target for invasion and annexation was Bohemia itself, a fear shared by both the central government and the Bohemian estates. Haugwitz's reforms showed considerable promise, but their impact was totally disrupted by the massive expenditures required by the Seven Years' War. From the end of that war in 1763 to the last year of Maria Theresa's reign in 1780, the consuming concern of the monarchy was to find the right combination of taxes, loans, and economic improvements to provide for the military and to service the debt. Dickson notes that the monarchy after the war sustained a good credit rating among European investors, because it paid the interest on its loans and a portion of the principal regularly and on time.

The greatest weakness of this study—a weakness that is truly frustrating—is its failure to offer clear answers to the obvious questions. In relation to the other great powers of Europe, was the monarchy rich or poor? Were its debts large or small, in relation not only to other powers (France and Prussia most notably) but to the financial facilities of the state? Was it a well run or poorly

run government? Was society stable so that no revolution such as occurred in France was likely in Austria, or did some emergency tax relief in the 1770s head off what might have become a revolutionary situation? Was the government enlightened or was it not (a weary old saw perhaps but one that must be addressed in any discussion of Central European government)? And, the crucial final question, was the Habsburg Monarchy between 1740 and 1780 a bona fide great power? Dickson implies that the evidence is too murky to answer these questions, but, after encountering so many statistics, so many lists, and so many tables, the reader cries out for the author at least to try. The great disappointment comes at the end, when the statistics and the data simply come to a stop, leaving the reader to ponder—even wonder—what it all means.

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Louisiana State University

MURRAY G. HALL. *Österreichische Verlagsgeschichte 1918–1938*. In two volumes. (*Literatur und Leben*, new series, number 28.) Cologne: Böhlau. 1986. Pp. 427; 435–600. DM 148 the set.

Murray G. Hall, a native of Canada who has lived in Vienna for fifteen years or more, has written a study of Austria's literary publishing houses in the early part of this century that provides a mine of encyclopedic information about a little-known but intrinsically fascinating field. This book is likely to become a standard reference work for the subject.

The two volumes complement each other effectively but can be used separately. In the first volume, Hall presents a multifaceted historical survey; the second volume is in effect a long appendix in which he describes at varying lengths the nature and contributions of over fourscore publishing houses, principally Viennese, in the interwar period. One might wish that each volume contained its own index, if not bibliography, but these, apparently thorough and helpful, are found in the second volume.

Ranging more widely than the title suggests, Hall begins with an examination of the legal and economic factors that limited the number and size of literary publishing firms in Habsburg Austria. The tradition of censorship and the refusal of the imperial government to subscribe to international copyright agreements were two such constraints. A major effect of these limitations—an effect that itself became a limiting factor—was the dependence of many of Austria's best-known writers on publishers in the German Reich. This relationship survived the Great War and contributed significantly to Austria's susceptibility to blandishments

and pressures emanating from Germany after 1933. In this section of the book Hall includes descriptions of eighteen pre-1918 publishers.

Austria's publishing landscape changed markedly after World War I. Indeed, for a number of legal, economic, and psychological reasons, conditions for several years after 1918 produced a surprising "mushroom atmosphere" that stimulated a dizzying proliferation of publishing firms devoted primarily to literary works. Because of inexperience and poor management, many of these ventures were short-lived, but they left a colorful and memorable legacy of controversial experimentation and achievement. Over time, unfortunately, old bureaucratic constraints were revived; even more ominously, the dominance of German houses remained essentially unbroken.

Hall devotes almost three-fourths of the first volume to developments after 1933. He looks both at censorial gestures and measures within Austria (some of them imitative or cosmetic) and at successive and generally successful efforts of the German government and of subservient German publishers to overwhelm Austria's economic and intellectual life. The neo-censorship of Kurt Schuschnigg's regime concentrated on "protecting" (sexual) morality, the church, and the "Austrian tradition" (including governmental authority), as well as the economic interests of an unimaginative "clique" of writers, publishers, and officials. Some of these connections survived World War II and influenced the post-1945 scene. In general, while clearly advocating freedom of expression, Hall compares Austria's record of censorship favorably with practices in Nazi Germany, and he evinces some sympathy for the governments of Engelbert Dollfuss and Kurt Schuschnigg in their struggle against German domination. But he laments the failure of Austria's bureaucrats to strengthen a domestic base for the publishing of worthwhile and enduring literary works.

One of the strengths of Hall's book is his detailed and well-documented treatment of relevant legislation and negotiations with German officials. He describes, for example, several inadequate attempts to provide support for Austrian publishers and the tedious, frustrating efforts to gain concessions from German agencies, most notably the newly created central publishers' clearing house in Leipzig, which controlled and restricted the importation of books published abroad. To good effect Hall elaborates on selected "case studies," such as the contrasting fortunes of the Bermann-Fischer Verlag and the Bastei-Verlag. Throughout the extended narration, Hall demonstrates a delightful, sardonic wit and a gift for telling one-liners, as when he suggests that Aus-

tria's cultural guardians were always "something special" and yet always something else besides, that is, well-connected insiders who "already had a number of other, not infrequently conflicting interests to represent" (volume 1, pp. 209–10). Even more trenchantly Hall calls the ineffective, corruption-ridden administration of legislation intended to support Austrian publishers "a scandal pure and simple" (volume 1, p. 218).

In his last, long chapter Hall examines the process of "uniting" Austria with the Nazi Reich, including the chaotic, destructive "aryanization" and "liquidation" of Vienna's many publishing firms owned by Jews. Here again Hall describes in text or notes the fate of selected firms and individuals, both the dispossessed, many of whom vainly tried to salvage their investments, and the Nazi opportunists who briefly flourished. He also highlights the conflict between inept, greedy Nazi careerists, on the one hand, and officials from both party and bureaucracy who were concerned about efficiency and order—even about "legality"—on the other. At the end of a traumatic transition, Germans, not Austrians, dominated a changed publishing scene, although some "nationalistic" firms survived *Anschluss* and war.

Hall promises a sequel about the story in the provinces. If it is as thoroughly researched and ably written as this book, it will be welcome. A sequel may not alter the general picture, but a volume treating the somewhat different history of publishers in the provinces and cataloguing provincial publishing houses and their post-*Anschluss* fate would complete an ambitious and impressive undertaking.

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ERWIN MATSCH. *Der Auswärtige Dienst von Österreich(-Ungarn) 1720–1920*. Vienna: Hermann Böhlau. 1986. Pp. 298.

As the title suggests, this is not a history of the Habsburg monarchy's foreign relations but rather a book of reference describing the structure and personnel of the Austrian, and later the Austro-Hungarian, foreign service. Thus, while one will find no analysis of the Kaunitz Coalition, one can check precisely where and when a Kaunitz or a Kaunitz-Rietberg served as a chief of mission abroad or in a ranking position in the foreign ministry in Vienna. Or one may look up just when diplomatic relations with Denmark or Venice were established (1691 and 1797, respectively), or who the envoys were, or when relations with Portugal were terminated ("March 16, 1916, in consequence of their rupture by Austria-Hungary" [p. 120]).

Erwin Matsch, who is an active member of the Austrian Republic's foreign service, most recently as ambassador to Libya, has compiled a book with all the virtues of a good reference work. It is reliable; Matsch in his preface quotes another author's "we prefer gaps to fairy tales" (p. 12) as one of the maxims guiding him. It is reasonably easy to use, thanks both to its organization and to its thorough index of names (although one might have wished for a subject index as well). And it is balanced and fair. After noting criticisms of Count Agenor Goluchowski's "'careful and reserved style,'" he adds that, while to some the foreign minister "was a model of moderation, who was keenly aware of the weaknesses of the monarchy, to others he seemed indolent and devoid of ideas" (p. 88).

Here, as elsewhere, the tone is factual and informative. The anecdotal surfaces only rarely, such as when he notes in connection with Gyula Andrassy's appointment as foreign minister in 1871 that the count, in his more revolutionary youth, had been sentenced to death *in absentia* for his role in the events of 1848, "so that he was later called '*le beau pendu*'" (p. 86).

But the drier style suits the topic, especially since we are so aware that, for all its talents and achievements, neither the empire nor its foreign service survived a war that, in Count Czernin's words, quoted here, had "no precedent either in history or in the files" (p. 89). Hence a certain melancholy suffuses even this book of reference, from its first dedicatory line, "*officio extincto*," to its final document. It reports, under the date of November 8, 1920, that, with the closing of the last Austro-Hungarian legation (in Switzerland), "my official duties have ceased." It is signed by Baron Ludwig von Flotow, once chargé d'affaires in Berlin and First Section Chief at the Ballhausplatz, who was that day ending his career as "Director of the Foreign Ministry in Liquidation" (p. 217).

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JAROSLAV PURŠ. *Changes in the Standard of Living and Nutrition of the Working Class in the Czech Lands, 1849–1879*. (Published on the Occasion of the Ninth International Economic History Congress in Bern, 1986.) Prague: Institute of Czechoslovak and World History of the Czechoslovak Academy of Sciences. 1986. Pp. 238.

In 1960, Jaroslav Purš wrote in *Historica* II (p. 213) that there existed no comprehensive study of "the whole system of inhuman exploitation" that "sucked dry and physically destroyed

tens of thousands of workers" during the nineteenth-century industrial revolution in the Bohemian crown lands of the Habsburg monarchy. Since then, Purš and other Czechoslovak historians have produced a plentiful literature on industrialization that shows how technology, entrepreneurship, public policy, and private capital helped transform "the Czech Lands" (Bohemia, Moravia, Silesia) into an advanced manufacturing region. Surprisingly, however, they have treated only piecemeal and sparsely such vital topics as working conditions, wage trends, and standards of living during the changeover.

With this monograph, Purš himself amply fills the scholarly gap he noted years ago. Adhering to "the thesis of the absolute impoverishment of the working class under capitalism" (p. 54), he relentlessly details the terrible impact of factory production in mid-century as people were converted from peasants and cottage handicrafters into urban proletarians subject to capitalist overlords and the vagaries of market forces. Purš properly does not distinguish Czech workers from Germans in this time; they often worked side by side and suffered mutually from political helplessness, lack of class consciousness, and worry over survival. The inevitable human consequences of an economic climate dominated by laissez-faire German liberalism were periodic joblessness, a diet of boiled potatoes, and rising accident, disease, and mortality rates. Working-class gains such as nominal wage increases and shorter hours won in boom years signified no rise in real wages (here, Purš is adamant) because they were negated by the increased cost of necessities and the intensified work time spent on more efficient machines. The workers, in short, became "stupified machine minders" (p. 60) consigned to history's dustbin.

Purš's findings place the Bohemian labor experience in line with the traditional orthodox Marxist interpretation of the travail of other European countries that endured the classic industrial revolution. His evidence comes from contemporary official records, newspapers, and memoirs as well as time-honored studies by Albín Bráf, Heinrich Rauchberg, and similar analysts. Regrettably, his study seems to be based on older research slightly updated for this publication. For example, his 450 reference notes and tables, which occupy one-third of his pages, include only ten sources that were published after 1960, apart from his own writings, and only one source after 1978. Thus he omits mention, among others, of Milan Myška's recent studies of social mobility in the Bohemian iron industry and Jiří Matějček's findings of a rise in real wages among foundry workers between 1850 and 1880. In concentrating on the misery of the multitude, he has scantied the elements of skill,

education, and income differentiation that were emerging among some of the workers in the period he examines.

This monograph will interest Anglo-American scholars because its English rendition opens to them a little-known realm of Central European economic history. It lacks a table of contents, an index, and a bibliography, but it is valuable for its command of primary sources, its statistical infrastructure, and its dramatic portrayal of the gross human costs exacted by the Industrial Revolution in the Bohemian crown lands.

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VERA ZIMÁNYI. *Economy and Society in Sixteenth and Seventeenth Century Hungary (1526–1650)*. (Studia Historica, Academiae Scientiarum Hungaricae, number 188.) Budapest: Akadémiai Kiadó. 1987. Pp. 119. \$14.00.

The history of Europe, as conceived in this country, is, in practice, generally confined to a fraction of what geographers mean by the term. Regrettably, the barriers of language and perhaps the complexities of cultures have kept our historians' interests confined, in the main, to those countries speaking Germanic or Romance languages. This tendency is even more pronounced in subfields of the discipline, such as economic history. Yet in principle all would probably agree that much insight could be gained from an extended vision, from a more thoroughly comparative approach. For this reason alone the appearance of this short monograph on the Hungarian economy in the sixteenth and seventeenth centuries must be viewed as a welcome addition to the small number of books on East European socioeconomic history accessible to Western scholars.

Hungary is, in actuality, a part of Europe, even if traditionally more backward than the lands to its west. From the Middle Ages onward, the growth impulses generated in the northwestern confines of the Continent have been propagated to the lands of the Danube as well. Therefore, in many respects, the secular development of the Hungarian economy conforms to the cycles experienced in other parts of Europe. Climatic changes affected its agricultural sector, and, through the supply to food, its population growth, as they did elsewhere on the Continent. Being part of the European disease pool, too, meant that Hungary's population growth pattern approximated that of the West. Because markets in the West were within reach (Paris and Budapest are separated from one another by only a few miles more than the distance between Chicago and New York), even if only with

difficulties, fluctuations in Western European demand were transmitted to Hungary through the trade in cattle, copper, wine, and precious metals. In turn, Hungary imported textiles, thereby establishing the character of its foreign trade that was to persist into the twentieth century. Moreover, through direct investment in the mining sector by German entrepreneurs, Hungary was integrated into the West European capital market and therefore influenced by conditions within it. (In 1566 one German mining enterprise employed two thousand workers [p. 19].) Hungary was also open to European intellectual currents. The rapidity with which the price revolution and agrarian boom of the sixteenth century reached Hungary is no less amazing than the speed with which Martin Luther's ideas reached the pulpits of its northern mining towns.

In short, the fact of having been integrated into Europe, not completely but nonetheless to a great extent, defined the main parameters of Hungary's socioeconomic development. The Turkish occupation of much of its territory notwithstanding, in the sixteenth century its population grew, its markets expanded, and its economy prospered, as was the case in most of the other areas of Europe. The increased prosperity is evident from the accumulation of jewelry in the estates of the nobility and even in the brisk market in castles. The average price of a castle is said to have risen by about 50 percent in the course of a generation (p. 70).

Of course, being part of Europe had its disadvantages as well. When silver from the New World began to flood into the Continent, Hungary's mining industry suffered correspondingly. Moreover, once the sixteenth-century boom was overtaken in the West by the crisis of the seventeenth century, Hungarian domestic markets shrank, foreign trade declined, and a century of stagnation ensued.

Although this book is valuable as a very brief introduction to the subject, it is not easy reading, because the translation is awkward in places. The work is also marred by a certain amount of confusion in describing the dynamics of social relations in the countryside. Thus, although asserting that during the sixteenth century agrarian boom landlords "encroached on the peasants' fields" (p. 31), on the following page Vera Zimányi adds, perhaps as an afterthought, that "the confiscations concerned about one to five per cent of the inhabited or deserted peasant tenancies." Hence, the reader is left with a needless sense of uncertainty about the actual course of developments. A richer analysis of the lord-serf relationship, albeit for Russia and for a later period, can be found in Arcadius Kahan's *The Plow, the Hammer, and the Knout: An Economic History of Eighteenth-Century*

Russia (1985). Nonetheless, all who choose to do so will certainly benefit from perusing Zimányi's contribution to the sparse literature on the economic history of early modern Eastern Europe.

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DOMOKOS KOSÁRY. *Culture and Society in Eighteenth-Century Hungary*. Budapest: Corvina. 1987. Pp. 249.

In Hungary, as in the rest of Europe, both East and West, the Enlightenment challenged established institutions and values in every area of public life. Perhaps this is the salient point to emerge from a reading of this shortened version of Domokos Kosáry's massive *Művelődés a XVIII. századi Magyarországon* (1980). In the present volume he is concerned primarily with tracing fundamental changes in the ways in which Hungarians viewed themselves and the world about them.

Kosáry sees the eighteenth century in Hungary as a period of gradual, fitful movement toward modern forms. The contradictory nature of that progress is particularly evident in economic development and social structure. He shows how the rhythm of agricultural production accelerated from the 1740s on under the stimulus of wars and the increasing demands of industrializing Austria for foodstuffs and raw materials, and he emphasizes that Hungarian agriculture remained dependent on the foreign rather than the domestic market. In contrast, Kosáry points out that industry failed to undergo any significant development during the century, but, unlike nationalist historians, he attributes this stagnation primarily to general problems of economic development rather than to Austrian "colonialism." Social structure also underwent change. Kosáry describes the growth of the urban middle class and the emergence of the lay intelligentsia, but he makes it clear that these events did not seriously affect the relative position of the various classes, since political and social leadership continued to be exercised almost exclusively by the great landowning nobility.

Against this background Kosáry examines the leavening effects of the Enlightenment on Hungarian intellectual and cultural life. He skillfully delineates the major political currents, showing how the nobility sought to adapt the prevailing order to certain new ideas and how, in contrast, the "anti-feudal" reformers, composed mainly of the new intelligentsia, aimed and failed to bring about a genuine overhaul of the political system. Of great interest, too, are the chapters on national awakenings, the churches, education, literature,

and the sciences. Kosáry offers the reader of English an introduction to cultural questions only touched on in the other main English-language works on the period; Henrik Marczali's *Hungary in the Eighteenth Century* (1910) and Béla K. Király's *Hungary in the Late Eighteenth Century* (1969). Kosáry describes how enlightened absolutism challenged the monopoly of the church over education and brought changes at all levels and among all classes and nationalities; he investigates the striking diversity and the mingling of genres and themes in literature from the classical to the Romantic; and he traces the evolution of scholarly disciplines such as history, where the Jesuits played a major role in the publication of sources and the writing of syntheses, and linguistics, where scholars, true to the spirit of the Enlightenment, sought to fashion and regulate the literary language.

This volume is an excellent introduction to eighteenth-century Hungary, but it is only an introduction. Our appetites are whetted but not satisfied. How much better it would have been for an understanding of Hungary and of Central Europe in general had the publishers made available in English the entire text of the author's 1980 work.

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BÉLA KÖPECZI, editor. *Erdély Története* [History of Transylvania]. Assisted by GÁBOR BARTA. In three volumes. Budapest: Akadémiai Kiadó. 1986. Volume 1, *A kezdetektől 1606-ig* [From Beginnings to 1606], edited by LÁSZLÓ MAKRAI and ANDRÁS MÓCSY. Volume 2, *1606-tól 1830-ig* [1606-1830], edited by LÁSZLÓ MAKRAI and ZOLTÁN SZÁSZ. Volume 3, *1830-tól napjainkig* [1830 to the Present], edited by ZOLTÁN SZÁSZ. Pp. 611; 617-1185; 1193-1945.

Since 1956 Hungarian historians, more than any comparable group in Soviet-dominated Eastern Europe, have gradually managed to rid their trade of the Stalinist legacy. Many have even ceased to write what C. A. Macartney called "tribal histories." In addition to some first-rate books, this trend can be seen in the immensely popular yet scholarly journal *História* and in the success of the Seventh International Congress on the Enlightenment held in Budapest in the summer of 1987. Yet the task is arduous, the challenge recurrent. The publication of a new, three-volume history of Transylvania is a case in point.

Published by the Institute of History of the Hungarian Academy of Sciences, this lavishly illustrated set includes among its dozen contribu-

tors some distinguished scholars of the interwar period, such as László Makkai and Zoltán Trócsányi, while the majority, mostly members of the institute, belong to a generation trained under Marxist auspices since the end of World War II. At the time of the work's appearance, general editor Béla Köpeczi, an expert on the Enlightenment and the anti-Habsburg uprising of Prince Francis II Rákóczi, was Hungary's minister of culture. The contributions reveal familiarity with the relevant primary sources and the multilingual secondary literature as well as professional competence.

Since prehistoric times Transylvania has been a land of many peoples. During the last millennium, ancestors of three ethnic groups in particular, Magyars, Germans ("Saxons"), and Romanians, played a dominating role in the development of this largely mountainous scenic region of South-eastern Europe, stretching to the west and north, respectively, of the eastern and southern Carpathians. Relying on rich archaeological, anthropological, and linguistic evidence, as well as medieval chronicles, the first half of volume I surveys the settlements of the earliest inhabitants of Transylvania, turning then to the Dacians, whose kingdom, destroyed by Emperor Trajan at the opening of the second century A.D., was incorporated into the Roman Empire. In 271 A.D., however, the Romans evacuated the province of Dacia to shorten their defense lines against the invading barbarians. According to Endre Tóth and István Bóna, there is no evidence suggesting the survival of any sizable Romanized population in Transylvania during the turbulent centuries of the "Great Migration of Peoples." Confirming this old thesis of Hungarian historiography, vigorously challenged by Romanian historians, who stress the significance of Transylvania in the ethnogenesis of the modern Romanian people on the basis of the theory of Daco-Romanian continuity, the new history of Transylvania describes the tenure of German (Gothic, Gepid), Turkic (Hun, Avar), and Slavic (Moravian, Bulgarian) peoples in the Danubian Basin and Balkans, contending that at the time of their conquest of the Carpathian Basin in the closing years of the ninth century the Magyars did not run into substantial military resistance and that the first area of the future Kingdom of Hungary occupied by them was Transylvania. Their first settlements rose in the valley of the river Mureșul (Maros) to protect the ancient salt mines; by the late eleventh century, similar Magyar military frontier settlements appeared in southern Transylvania, to be known as the land of Székelys.

Although Makkai's interpretation of the formation and chronology of the first Székely settle-

ments differs from Bóna's in his magisterial chapter on Transylvania's place in the medieval Hungarian kingdom, he agrees with the other authors that of the largest ethnic groups currently living in the Carpathian Basin only Slavs had arrived there prior to the Magyars; the first Transylvanian Saxons came in the twelfth century, while the first documentary references to Romanians emerge only in the thirteenth century. But, unlike the older nationalistic school of Hungarian historians, the authors of the new history of Transylvania refrain from deriving special "historic" rights from this chronology. By the same token, the "Latin mythology," to use Hugh Seton-Watson's felicitous term, in which Romanian historians prefer to believe but which cannot be proven for certain "missing" centuries, conveys no special privileges in Transylvania, a country that has been genuinely multicultural at least since the fifteenth century, if one keeps in mind the Saxons' special contributions and the rise of assimilated local Romanian leaders into the Hungarian nobility, as exemplified by the Hunyadi family, while the ethos of the popular masses remained steeped in Orthodoxy.

Although Transylvania was one of the lands of the Hungarian crown, it had a separate status within the feudal medieval state. By the fifteenth century, its foremost royal dignitary, the *voevod*, originally in charge of Romanian settlers, became one of the great barons of the realm. The same century also witnessed the emergence of the three feudal estates constituting the three privileged corporate bodies of political Transylvania, namely, the Magyar nobility, the free Székely frontier guards, and the Saxon *universitas* of burghers and free peasants. These three political "nations" were not defined on the basis of ethnicity; they excluded all serfs, Magyars and Romanians alike.

The unique features of Transylvania as an independent state began to take shape following Hungary's defeat by the Turks in 1526. The Turkish occupation of the central area of the country led to the establishment of a Transylvanian client state under the tutelage of the Sublime Porte. "Royal" Hungary was confined to a few counties in the western and northern parts of the medieval kingdom held by the Catholic Habsburgs. Most of Transylvania's ruling families embraced Protestantism: the Saxons became Lutherans, and many of the Hungarian landowners followed the Calvinist creed. Roman Catholicism was "saved" by Prince Stephen Báthory, who was also elected king of Poland in 1575. By that time Unitarians had received equality with the followers of the three main branches of Western Christianity, and even "Judaizing" Sabbatarians enjoyed

a measure of toleration for awhile. In the age of religious wars in Western Europe, this was a remarkable feat, under the often burdensome and dangerous but nonetheless protective shield of the Ottoman superpower. Since, however, Eastern Orthodoxy was not one of the four privileged creeds, most Romanians were left out of the "union" of three nations and four religions constituting the institutional framework of the principality of Transylvania.

The last chapter in volume 1 and the first three in volume 2 deal with independent Transylvania between 1526 and 1711. The periodization raises questions about the structure of the entire work. The transfer of Transylvania from Ottoman to Habsburg rule occurs in 1691 although the inclusion of the anti-Habsburg uprising of Prince Francis II Rákóczi (1703–11) in this section is arguable. The exclusion of the formative eight decades from Mohács to the Peace of Vienna (1606) is not.

The consecutive chapters on independent Transylvania by Gábor Barta, Katalin Péter and R. Ágnes Várkonyi deal with institutional and economic developments, religious affairs, social conditions, the shifting balance of power on the international scene, and the ruthless infighting of the baronial class. Remarkable portraits of farsighted leaders with human frailties alternate with descriptions of the meager material and rich spiritual resources of the common people, serfs, artisans, miners, clerics, teachers, and merchants of different ethnic backgrounds. Social tensions and misery are presented as part of the human condition, generated by greed and misfortune. Beautifully written, these chapters capture the spirit of two centuries of Transylvania's exposure to diplomatic, military, and cultural impulses from two imperial capitals, Constantinople and Vienna, and of its efforts to reach out to the Protestant powers of the west and north, to Catholic Poland, and even to Orthodox Muscovy to survive the conflicts erupting over the borderlands of clashing civilizations.

Similarly learned and fair are the chapters on the "Grand Duchy" of Transylvania under the Habsburgs, covering the period 1711–1848. The small country had ceased to be an important link in the East European security system of France; however, contacts with the Protestant maritime powers and Swiss and German lands continued through the exchange of students, clergy, and teachers. The constitutional fiction of restoring a Hungarian national kingdom via Transylvania, attached to the ablest of the principality's leaders such as a Bethlen, faded away. Princely despotism was replaced by royal absolutism operating through the Transylvanian Court Chancery in Vienna and the governor and gubernatorial coun-

cil and diet in Transylvania. The internal autonomy of the grand duchy was confirmed and its separation from the other Hungarian crown lands maintained until the Compromise of 1867 except for brief intervals during the centralizing efforts of Joseph II and the Hungarian revolution of 1848, which proclaimed the union of Transylvania and Hungary.

Habsburg control of Transylvania strengthened Roman Catholicism in the Grand Duchy. One of the consequences was the formation of a Uniate church, which weakened Romanian Orthodoxy at a time when Romanians became the absolute majority of the population. Ironically, the high clergy of the dissident Uniate church, who studied in Vienna or Rome, contributed greatly to the awakening of their compatriots' consciousness, striving, in the words of Keith Hitchins, "by every means possible to prove the Roman origins of the Rumanian people and the Latin derivation of their language."

The birth of the idea of a Romanian nation in Transylvania did not lead to admission of Romanians to the "union of three nations and four religions," that is, to political Transylvania. But it coincided with the unfolding of enlightened ideas in that land. In one of the best chapters of the work, Trócsányi describes the simultaneous penetration of the Enlightenment into the Saxon, Hungarian, and Romanian cultural-national communities of Transylvania, the relationship of this process to Vienna's enlightened absolutism, and the contrast between educational-cultural progress and socioeconomic backwardness, manifested, *inter alia*, in the absence of urban regulations in Transylvania and in the largely Romanian peasant uprising of 1784, led by Horea and Cloșca. This prelude to the age of reform and national awakenings in both Hungary and Transylvania, which took shape during the early 1790s but was then subdued by the political reaction under Francis I and the post-Napoleonic conservative restoration, belongs with the meticulously researched and well-balanced chapters by Ambrus Miskolczy on the 1830s and 1840s including the revolution and civil war of 1848–49.

These chapters and the subsequent three written by Zoltán Szász on the periods of neo-absolutism, dualism, and post-World War I revolutionary upheavals (1849–1919) constitute the bulk of volume 3. They deal with the growth of the increasingly self-conscious movements of nationalities, their often violent clashes, the complex transition from a feudal to a capitalistic order of society and from absolute to constitutional government, and the immediate consequences of a global military conflict on the territory of the Austro-Hungarian Monarchy, specifically Transylvania.

The presentation is scholarly and intellectually honest. Given the nationalistic traditions of pre-World War II Hungarian historiography, this is an impressive achievement although it stops short of István Deák's verdict that the terrible Transylvanian ethnic war in 1848–49 "should and could have been prevented." By the same token, the responsibility for all the missed opportunities for reconciliation between the Magyars and Romanians of Transylvania after the latter's incorporation into Hungary following the Compromise of 1867, which also put an end to Saxon autonomy, must be placed on the Hungarian leadership.

The principle that those in power bear primary responsibility cannot be limited to the period preceding World War I. Perhaps inescapably but certainly regrettably, the sketchy chapter concluding the text tries to survey "Transylvania's road" under "capitalist" and "socialist" Romanian regimes since 1918. Instead of a detailed analysis or a panoramic view, the outline of seven decades on forty-six pages, Köpeczi admits, is incomplete. To what extent this is attributable to the inaccessibility of basic archival sources even for the interwar era, let alone the period since 1944, or to conflicting political interests an outside observer cannot tell. Since the disintegration of Austria-Hungary, all Hungarian and Romanian governments have had many reasons to keep substantial portions of their archives closed to researchers. The circumstance that the author of the section on the most recent periods of Transylvanian history was at that time a member of the government renders the entire work more vulnerable, because it raises questions about its political inspiration and purpose.

Indeed, few recent books printed in Eastern Europe have received the kind of publicity given to the work under review. Romanian Communist leader Nicolae Ceausescu's public attack on those who fail to recognize the Romanians' descent from the Romans in accord with the theory of Daco-Romanian continuity, which has dominated Romanian historiography since the end of the eighteenth century, was followed by an anonymous full-page advertisement in the April 7, 1987, issue of the London *Times* accusing the Hungarian Academy of Sciences of "a conscious forgery of history." Summarizing an article written by a team of Romanian historians led by academician Stefan Pascu, the advertisement rejected "the attempt to locate the formation of the Romanian people only south of the Danube," insisting on Romanian continuity north of the river "in the intra-Carpathian-Danubian-Pontic space" and on "the Romanian people's legitimate rights over its ancestral hearth," that is, Transylvania, and on their recognition "as defenders of the European civilization and Christianity" in the wars against the Otto-

mans. It also criticized "the distorted presentation of the political status of Transylvania in various historical periods," denouncing the "Budapest historians" for resuming the revisionist and chauvinistic theses of interwar Hungarian historiography, for ignoring "Transylvania's organic integration in the Unitary Romanian National State" after 1918, for falsifying "the aspects related to the deportation and massacre of the Jewish population of northern Transylvania after the Vienna Award of 1940," and for misleading public opinion regarding the Romanian state's "definite" solution of the national question "in the years of socialism." The critique concluded with a reminder of the failure of Hungarians in their fight against socialism in 1956, implying that the present book was but another aspect of this fight "against socialists" and that it was related to the Hungarian government's recent effort to raise the question of the Hungarian minority's rights in Transylvania at the Vienna conference on human rights, a maneuver whose impact on the "western world" was yet to be seen, as was "the way the Russians will swallow it."

Pointed out in slightly polemical articles published in the *Wall Street Journal* (May 19, 1987) and the *Times Literary Supplement* (October 2–8, 1987), the absurdities of the Romanian critique of the Hungarian history of Transylvania are nevertheless important. Beyond indicating the growing tension between the two neighboring "fraternal" Communist regimes, reminiscent of the frictions that led to the second Vienna Award in 1940, which divided Transylvania between Hungary and Romania in the first phase of World War II, the publication of the massive Hungarian study and the vehement but predictable Romanian reaction to it have proven again that, notwithstanding the predictions of the *Communist Manifesto* and Marxist-Leninist theoretical writings, Soviet-type socialism does not offer a ready-made solution to the disruptive menace of clashing linguistic-ethnocentric nationalisms in either East Central Europe or Soviet Russia.

Like their Romanian colleagues, Hungarian historians have the right to write professionally about any aspect or period of Transylvanian history. The challenge of a joint past is there; its glory is interwoven with horrors and holocausts alternately shared and committed. There is enough blame to go around and achievement to be proud of. Hungarian culture would be much poorer without its Transylvanian component. Miklós Olah (Nicolaus Olahus), son of a Wallachian boyar born in Transylvania and a humanist friend of Erasmus, became archbishop-primate of royal Hungary. His contemporary, Gáspár Heltai (Kaspar Helth), a Lutheran pastor of Saxon birth

who later embraced Calvinism and ended up in the Unitarian faith, was the first to undertake the full translation of the Bible into the Magyar vernacular and also had an immense influence on the development of Magyar orthography and fiction with his printshop in Cluj (Kolozsvár, Klausenburg). The two Bolyais, Farkas and János, father and son, early nineteenth-century Hungarian pioneers of modern mathematical thinking, hailed from Transylvania, as did the novelists Baron Miklós Jósika and Baron Zsigmond Kemény and the greatest of twentieth-century Hungarian poets, Endre Ady. The first Magyar language permanent theater opened in Cluj (1821); indeed, there is no need to offer further proof to indicate why Hungarian historians find it difficult to ignore this particular part of their national culture. Nor is there a need to do so as long as they recognize contributions of other nations in the same region.

Many parts of the new history of Transylvania constitute a treasure house of information. Some take a legitimate stand on controversial historical issues that may never be resolved to the satisfaction of a scrupulous historian. Sadly, it does not augur well for the historiography on East Central Europe that such a collective enterprise involving a multicultural historical entity could not be undertaken as a cooperative venture of historians of neighboring and other countries but becomes a point of departure for additional ideological controversies.

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WOLFGANG-UWE FRIEDRICH. *Bulgarien und die Mächte 1913–1915: Ein Beitrag zur Weltkriegs- und Imperialismusgeschichte*. (Quellen und Studien zur Geschichte des östlichen Europa, number 21.) Wiesbaden: Franz Steiner. 1985. Pp. xx, 453. DM 78.

This monograph offers a thorough investigation of Bulgarian policies leading to Bulgaria's wartime entry on the side of the Central Powers in 1915. Wolfgang-Uwe Friedrich has examined all of the major European state archives of those nations seeking Bulgarian assistance, and, unhampered by a language barrier, has also sifted the pertinent sources in Sofian repositories. A carefully researched and comprehensive book, it may well prove the authoritative treatment of the subject.

The initial section considers internal Bulgarian institutions: the political party structure, the position of the king and cabinet, and the importance of the army. Friedrich points to the tendency for the parties to splinter, which necessitated sixteen general elections between 1879 and 1914. But after 1908 when Ferdinand became king, he relied

on coalition cabinets and then chose Vasil Radoslavov in 1913 as prime minister because of his loyalty to the crown and his capacity to manipulate political groups. The army officer corps proved a potent force for expansionism.

The next segment concentrates on Bulgaria's shift to a revisionist policy following its defeat in the Second Balkan War. To be successful, this policy required transferring allegiance from Russia to Austria, reconstructing the army, and reviving the Bulgarian economy. Reconstructing the army and reviving the economy necessitated a foreign loan of 500 million francs, which Radoslavov was not averse to seeking in Paris and St. Petersburg. Both Entente powers, however, proved wary because of Bulgaria's uncertain political loyalties and poor economy, and, when their financial circles demanded virtually a monopoly of Bulgaria's lucrative tobacco industry, Sofia turned successfully to Vienna and Berlin. German and Austrian demands were harsh enough to be rejected by the Bulgarian *Sobranje*, but the loan was signed as a royal ukase in mid-July 1914. Ferdinand called the agreement the "fruit of French-Russian vindictiveness."

The rest of the work covers Bulgarian diplomatic relations with the Entente and Central powers from August 1914 to the Bulgarian entry into war in September 1915. Friedrich's coverage of negotiations with Germany and Austria discloses nothing new, but what is unique is his full and comprehensive examination of Bulgarian-Entente discussions. The problem centered on Bulgarian territorial demands, which would have meant considerable sacrifices by Serbia, Greece, and Romania. The Entente never resolved its dilemmas. Serbia was an active ally, Greece and Romania were close friends of France and England, and the Bulgarian-Russian rift could not be closed. Negotiations were continued, however, because Radoslavov maintained his policy of neutrality, and in this he was backed by his parliament and king.

Although the Entente did offer Bulgaria some territorial concessions, they proved insufficiently extensive and could not be guaranteed. In the end the Allies were defeated diplomatically by Balkan irredentism, the failure of the Dardanelles campaign, a Russian setback on the eastern front, Bulgarian conviction that Germany would win the war, and the willingness of the Central powers to guarantee the enlarged Bulgaria originally created by the Treaty of San Stefano in 1878. With its "national idea" virtually realized, Sofia declared war against the Entente.

One must hope that this excellent study will be further pursued to cover Bulgarian activities from

1916 to the final capitulation at Neuilly in 1919. The gap remains.

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ANGELA T. PIENKOS. *The Imperfect Autocrat: Grand Duke Constantine Pavlovich and the Polish Congress Kingdom*. (East European Monographs, number 217.) Boulder: East European Monographs; distributed by Columbia University Press, New York. 1987. Pp. xiii, 186. \$25.00.

The Grand Duke Constantine has been a "gray blur" in both Polish and Russian history, and, unlike the other "gray blur," he has remained gray for generations of historians. He is known as a brutal martinet who inexplicably preferred life in Poland to occupying the Russian throne, although his direct influence over both countries was minor. Angela T. Pienkos fleshes out this vague portrait in a brief but useful biography. She concentrates on Constantine's importance in defining Russia's overall relationship with Poland, rather than providing a detailed study of his actions.

Constantine was the second son of Emperor Paul and the grandson of Catherine the Great, who treated him like his more celebrated brother, Alexander, by preparing him, unsuccessfully, to rule as a philosopher-king. With the failure of the empress's grand scheme to make Constantine emperor in Constantinople, the grand duke entered the army and served with some distinction in the wars against Napoleon. Like Alexander, he favored the creation of a Polish state at the end of the war as part of the Russian empire.

In 1815, Constantine became the commander-in-chief of both Russian and Polish forces in Poland and gained extralegal but definitive control over the constitutional government. He encouraged conservative, yet distinctly Polish, support for the Russian regime and distanced himself from Russians who wished to suppress Polish individuality, such as Governor-General Novosiltsev, as well as from Polish liberals. Solidifying his control over the country, Constantine rebuilt the Polish army (along Russian lines) and may even have aimed at reunifying the lands lost in the First Partition with the Congress Kingdom. Nevertheless, his hopes of building support were largely in vain, because he alienated conservative Poles by choosing unimaginative and reactionary subordinates instead of allying himself with conservatives of vision (such as Prince Adam Czartoryski). The only conservative of standing who flourished under Constantine was Prince Ksawery Drucki-Lubecki.

Nicholas's assumption of power in 1825 limited

Constantine's autonomy considerably. As Nicholas made it clear that he would make no more concessions, Polish feeling became so inflamed that an explosion became inevitable and, indeed, occurred with the insurrection of 1830. Brushed aside by both sides, Constantine could propose no alternative path. He left Poland shortly after the insurrection broke out and died in the cholera epidemic of 1831.

In general, Pienkos finds Constantine far more influential in determining the character of the Congress Kingdom and Polish-Russian relations than have previous historians and more constructive in his vision of an autonomous Poland. Although she may exaggerate his importance somewhat, particularly in view of his limited mentality, she has succeeded in filling in another blank spot in our knowledge of the period.

The book is well researched from archives in Poland and France. An appendix presents the translated texts of the constitution of the Congress Kingdom with an 1825 addendum and the Organic Statute of 1832.

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ELKE HAUSCHILDT. *Polnische Arbeitsmigranten in Wilhelmsburg bei Hamburg während des Kaiserreichs und der Weimarer Republik*. (Veröffentlichungen der Forschungsstelle Ostmitteleuropa an der Universität Dortmund, Reihe A, number 47.) Dortmund: Forschungsstelle Ostmitteleuropa. 1986. Pp. xv, 330.

Rarely has the history of a Polish community in Germany been probed in such elaborate detail as in this study of Wilhelmsburg, a suburb of Hamburg. The community grew gradually following the initial recruitment of labor for harbor construction in 1888 and of woolen workers for a new factory established in 1889. By 1914 Poles accounted for about 20 percent of the population of thirty-three thousand.

Insofar as the copious but uneven source material allows, Elke Hauschildt deals in painstaking detail with the standard questions of migration history. Her careful treatment of the evidence, scrupulous avoidance of excessive claims to originality, and frequent comparison of her findings with those of other studies on Polish migration, especially Richard C. Murphy's *Gastarbeiter im Deutschen Reich: Polen in Bottrop* (1982) and Christoph Klessmann's *Polnische Bergarbeiter im Ruhrgebiet 1870–1945* (1978), reinforce confidence in her conclusions. She traces developments in the age, sex, and occupational structures of the settler community, in working and housing conditions, in

patterns of social behavior, in the relationship between national and social loyalties, and in German perceptions of the Poles. The story she records is generally one of success, at least as reflected in a modest degree of upward social mobility, in the decline of hostile press perceptions, and in increased intermarriage after World War I—although the impact of the war itself on the community receives curiously little attention.

The core of the book concerns the role of the Catholic church. The tensions between Polish laity and German pastors are illuminated in lively detail. The vociferous demand of the migrants for a Polish priest reflected an explosive mixture of cultural, religious, and political impulses and sharply reduced the job satisfaction of the German clergy condemned to cater to their obstreperous Polish flock. It would have been interesting to have learned more about the attitude toward Poles of the small number of German lay Catholics in this predominantly Protestant neighborhood.

If the study confirms more than it challenges existing assumptions, it provides much useful detail to sustain, and sometimes to refine, accepted generalizations about the Polish migrant experience. The electoral behavior of the Wilhelmsburg community, for instance, differed considerably from the more familiar experience of the Ruhr. The tendency to support Social Democratic rather than Center candidates suggests that the electoral performance of Poles was by no means a prescribed ethnic constant and was significantly influenced, as was the case in other cities, by the specific local electoral environment.

There is an unnecessarily defensive attempt to insist on the relevance of the conclusions to the study of the more recent *Gastarbeiter* question. The study deserves to stand in its own right as an instructive survey of a small but interesting community that experienced in miniature many of the familiar pressures of migration, and whose history, so methodically recorded by Hauschildt, provides further testimony to the resilience of migrants in responding to changing circumstances.

J. J. LEE
University College Cork

CZESŁAW MADAJCZYK. *Die Okkupationspolitik Nazi-deutschlands in Polen 1939–1945*. Translated by BERTHOLD PUCHERT. Berlin, G.D.R.: Akademie. 1987. Pp. xiii, 702.

The Black Book, published in 1942 by the Polish government-in-exile, opens with a passage from the papal encyclical of October 1939 that speaks of a *hora tenebrarum*, a dark hour, “in which the spirit of violence and discord is pouring a bloody cup of

nameless sorrows over humanity.” In the Second World War, no people other than the Jews were more heavily or wantonly afflicted than were the Poles. *The Black Book* was a detailed indictment of Nazi occupation policy and a bill of particulars of German crimes in Poland in the twenty-two months from October 1939 to June 1941.

After the victory, a vastly more comprehensive body of evidence became available, but by then one kind of colonialism had been replaced by another, and the wartime experience had generated disturbing political implications for the Polish Communist government and its Soviet sponsor. A dozen years passed before scholars, Czesław Madajczyk among them, were permitted to begin publishing articles and monographs dealing with aspects of the occupation. In 1970, possibly with an eye to the then-burgeoning Soviet-West German détente, the State Publishing House in Warsaw published the first comprehensive work on the occupation, Madajczyk's *Polityka III Rzeszy w okupowanej Polsce*. The present volume, published in East Berlin, is a translation of that two-volume work, condensed and partly rewritten by the author, whose introductory comments suggest that it is also meant to be a modest excursion into *glasnost*.

Following essentially the same format that was adopted in the earlier work, Madajczyk has refined and expanded the indictment initiated in *The Black Book*. In six chapters, he analyzes the premises, the objectives, and the shifts in German occupation policies as they applied to the whole nation and to the subdivisions, the German-administered territory (the Government General), and the districts annexed to Germany (the Wartheland, Danzig-West Prussia, and the Białystok district). Nineteen chapters treat by categories the methods employed to obliterate the Polish nation and “exterminate” its people, and six assess the economic damage in its various aspects. The research is thorough, and the documentation is precise. Unfortunately, the author does not draw any more profound conclusion from the massive record of suffering and oppression he has compiled than that it justifies the postwar expulsion of the Germans from the German territories assigned to Poland under Josef Stalin's redrawing of the German-Polish boundaries.

The ventures into *glasnost* take two forms: recognition of the non-Communist resistance as an effective force in the wartime struggle for national survival and acceptance of the almost three million Polish Jews who died in the ghettos and gas chambers as an integral part of Poland's sacrifice. That openness has limits, however, is apparent. The minuscule but Communist *Armia Ludowa* gets far more credit than it earned, the non-communist *Armia Krajowa* less than it deserves and an impli-

cation that it collaborated with the Nazis in its 1944 *Burza* operations. The previous, longstanding indifference to the Jews is passed off as a consequence of German-imposed segregation that supposedly concealed their fate.

EARL F. ZIEMKE
University of Georgia

WŁADYSŁAW T. KULESZA. *Koncepcje ideowo-polityczne obozu rządzącego w Polsce w latach 1926–1935* [Ideological-Political Concepts of the Ruling Camp in Poland in the Years 1926–35]. Wrocław: Zakład Narodowy imienia Ossolińskich-Wydawnictwo, 1985. Pp. 310. 300 Zł.

Józef Piłsudski governed Poland as dictator on two occasions: the first time (1919–21) legally and the second time (1926–35) by virtue of a military coup. After the March 1921 Constitution deliberately consigned the executive—expected to remain Piłsudski—to political impotence, Piłsudski refused to run in 1922 for president. He went into temporary political retirement, much as Charles de Gaulle (who in this period served as a military officer-instructor in Poland) did a quarter of a century later in similar circumstances.

Piłsudski's May 1926 military coup ushered in the second period of his dictatorship, which ended only nine years later with his death in May 1935. Throughout this period, the Piłsudskiites labored to restructure Poland's parliamentary system into one in which power was exercised by a government nominally answerable to a parliament whose chief task was to enact budgets. The restructuring was crowned by the April 1935 Constitution, which had been tailored expressly for Piłsudski. It was the last public document that Piłsudski signed before his death the following month. The Constitution of 1935 inaugurated an elitist senate and a president who was in theory independent of the other branches of government and was empowered to issue decrees that had the force of law. Henceforth, the interests of the state, not the interests of the dozens of parties and numberless political coteries, were supposed to determine the policies of the government, as expressed by the president, who was, in turn, supposed to reconcile the various quarreling parties and factions.

The twenties and thirties marked the ascendancy in Italy, Germany, Spain, and elsewhere of antidemocratic, fascist forces. As Poland under Piłsudski in 1926–35 drifted ever farther from Montesquieu's model of parliamentary government, many Poles and foreigners believed Piłsudski was also becoming fascist. Hitler openly admired him and wished to meet him.

Władysław Kulesza, however, argues that

Piłsudski's dictatorship was not fascist but authoritarian. "Every fascism is an authoritarianism, but not every authoritarianism is a fascism" (p. 272). An authoritarian system is a dictatorship that leaves intact certain "spheres of freedom" and is thus a system of limited pluralism. Piłsudski's authoritarian rule had as its ultimate goal the education of the citizenry, particularly of the young, to their commensurate obligations to both common weal and their individual interests.

Whether Piłsudski, like de Gaulle, had a steadfast political ideology can be debated. Piłsudski's progressive illness during the early 1930s, which culminated in his death in 1935, essentially stopped Poland's political evolution. His men—Marshall Edward Rydz-Smigły, President Ignacy Mościcki, and Foreign Minister Józef Beck—all tried to carry out his will. Beck even stopped his office clock at the hour of "Grandfather's" death. Unfortunately for the Polish people, in a time when all spheres of opinion and counsel were needed, a political vacuum developed in Polish politics leaving Poland isolated politically and geographically, facing a Hitler-Stalin alliance in 1939.

Kulesza's book is worthy of publication by the Ossoliński Institution, with its long tradition of excellence in pre- and postwar historical scholarship.

RICHARD WOYTAK
Chapman College

MAUREEN PERRIE. *The Image of Ivan the Terrible in Russian Folklore*. (Cambridge Studies in Oral and Literate Culture, number 14.) New York: Cambridge University Press, 1987. Pp. x, 269. \$42.50.

Can we extract information about historical events and "mentalities" from folklore? Is folklore a guide to "popular attitudes"? What does the prominence of Ivan the Terrible in folklore reveal about the reign of Ivan and about the history of the Russian people more generally? These are some of the intriguing questions Maureen Perrie raises in her engaging essay.

The strength of the book is in its sweep. The materials covered include historical and popular songs, prose tales and other genres of folklore, as well as historical accounts. The book opens with a lively essay on the historiography of the question, and it concludes with a hundred and fifty pages of translations. What stands out is the richness of the treatment of the issue of the rulers and the ruled in Russian folklore, and the seriousness with which historians have regarded this material. The tragic dilemma seemingly posed by the apparently favorable image of Ivan in folklore was raised by

N. M. Karamzin, who complained that the tsar's conquests had erased his evil deeds in popular memory.

The author of the book traces her interest in the image of Ivan the Terrible to a dissertation on the Socialist Revolutionary party. The folkloric representation of Ivan, she argues, represents an early example of "popular monarchism" (p. ix). Folkloric images of Ivan were already favorable by the early seventeenth century. The purpose of the book, she explains, is to compare folklore about Ivan with authentic sixteenth-century sources. The essay, however, extends far beyond this to the object of her original curiosity.

The author's argument is straightforward. Ivan the Terrible was the first tsar to be positively presented in folklore, and he remained popular in several folk genres throughout the nineteenth and twentieth centuries. Although Ivan seems to us a nasty tsar, she argues that he was popular because there was a font of good feeling for monarchs. Ivan was regarded largely with approval in his own time, and the events of the seventeenth century made his reign seem like a golden age. Ivan became a mythogenic figure, expressing Russian attitudes toward their rulers, and his image has remained constant over the last two hundred years. Ivan, she suggests, benefited from the people's wish to see a big boss lord it over the little bosses they encountered in their daily lives. Ivan, in this sense, foreshadowed Joseph Stalin. "Tyranny and terror can have a certain popular appeal," she concludes (p. 117).

Despite the interest of this treatment, the essay lacks a certain rigor. The author is aware of various images of Ivan in various types of folklore, but in the end lumps them all together to signify a popular *mentalité*. But did such a thing in fact exist? What were the functions of the different genres? How widely were they known? How often were they performed and for whom? Certainly, the presence of Ivan in Russian folklore is incontestable, but how much of what was collected was representative either of oral culture generally or of popular beliefs at any time? These are admittedly difficult questions, but the author would have done well to wrestle with them more diligently. Behind the notion of a dominant message is the assumption of a "popular mind." Yet the Russian population was diverse. It is too easy to consider the collected folklore to be the expression of a unified oral culture that exemplified the common thinking of a people. The equation that matches a positive image of a tyrant in folklore with a popular sympathy for tyranny over several centuries of Russian history is questionable.

Who were the heroes and heroines of Russian folklore? How were tsars and princes character-

ized more generally? Were they usually brutal tyrants, whose tyranny was cited with approval, or was Ivan the exception? The author compares Ivan's image with that of Peter and recognizes regional variations. Yet the urge to simplify and synthesize wins out.

This is a stimulating book, which can be read with pleasure and with great benefit, but the author raises many more questions than she answers. Popular monarchism has been discussed by a number of scholars interested in modern Russian politics and society, including, among others, Daniel Field, John Bushnell, N. V. Riasanovsky, and Allan K. Wildman. It is tempting to see signs of the origins of such beliefs in folklore about Ivan the Terrible, but more evidence is needed. The author challenges us to think about these issues.

The book appears in the series Cambridge Studies in Oral and Literate Culture, and it is a pleasure to read. It would fit well into a year-long course on Russian history.

JEFFREY BROOKS

University of Minnesota

DANIELA NEUMANN. *Studentinnen aus dem Russischen Reich in der Schweiz (1867–1914)*. (Die Schweiz und der Osten Europas, number 1.) Zurich: Hans Rohr. 1987. Pp. 270. 38 FR.

As the holder of the Chair of East European History at the University of Zurich, Carsten Goehrke has given a new direction to the study of Russian history in Switzerland. Under his guidance, the Historisches Seminar of the university has embarked on an ambitious program of collecting information on the history of the Swiss in Russia and in the Soviet Union and has begun a publication series on this general theme. The work in hand, Daniela Neumann's dissertation, which was just accepted in the summer of 1987, has appeared as volume 1 of another series entitled *Die Schweiz und der Osten Europas*. Together these works are adding up to a substantial library on the history of Swiss-Russian relations.

Russian students, both male and female, constituted a surprisingly large percentage of the student bodies at all Swiss universities before the First World War. In days when American state universities contemplate the problems of finding places for out-of-state students, it is interesting to note that, in the period from 1902 to 1907, students from imperial Russia made up 28 percent of the student body at the University of Zurich, 38 percent at Bern, and 40 percent at Geneva. In Bern, women from Russia made up 80 percent of the female student body in this period, and, in Geneva, 83 percent.

Neumann's study focuses on the female students from Russia as a cultural phenomenon in their own right. She spends a considerable part of the study, about 30 percent of the text, examining the reasons they might have chosen to leave Russia to study in Switzerland during the half-century she has investigated.

Neumann points out that many of these women returned to Russia to participate in the revolutionary movement there, and she then concentrates more on their social adjustment to their Swiss hosts and within the Russian community than she does on questions of political organization or their academic organization and studies. This results in a narrative emphasizing individual experiences rather than in an account of the evolution of the life of the students.

Somewhat frustrating to me was the author's announced decision to refer to all women coming from Russia as *Russinen*. This led her to distinguish *judische Russinen*, but she did not consider *polnische Russinen*. Her interest, however, is in studying these students as women rather than as members of any given nationality.

In all, the work should be useful to anyone concerned with the study of Russian emigration in general, with the problems of Russian women in finding their own place in society before World War I, and with the history of the Russian revolutionary movement. The work has been printed from camera-ready typed copy, and it includes six tables, one chart, a bibliography, and over fifty pages of biographies of women who studied in Switzerland.

ALFRED FRICH SENN
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DEBORAH HARDY, *Land and Freedom: The Origins of Russian Terrorism, 1876-1879*. (Contributions to the Study of World History, number 7.) Westport, Conn.: Greenwood, 1987. Pp. xiii, 212. \$37.50.

Deborah Hardy is concerned in her book with the creation and disintegration of *Zemlia i Volia* (Land and Freedom), one of the first revolutionary organizations in Russia to formulate a coherent political program. Established in 1876, the organization sought to coordinate efforts to spread propaganda in the Russian countryside with the ultimate aim of inciting a peasant revolution. Barely three years later, however, the *Zemlia i Volia* collapsed, with a majority of its members adopting political terrorism as a tactic to force the government to grant civil and political freedoms and possibly a constitutional and parliamentary form of government. In two years the terrorists succeeded in assassinating the tsar, but his successor

simply arrested the terrorists and drastically hampered revolutionary action for many years.

This story has been told before in many histories of the Russian revolutionary movement, most thoroughly by Franco Venturi in his *Roots of Revolution*. But Hardy brings to her narrative many insights that help clarify the reasons for this "turn to terrorism" in the late 1870s. It is Hardy's thesis that the radicals of *Zemlia i Volia* did not abandon propaganda in the countryside because their efforts were unsuccessful or because they felt an overwhelming sense of frustration and despair. Nor was this the consequence of any carefully reasoned assessment of the efficacy of various forms of revolutionary action. Rather, their decision served to satisfy psychological needs that could not be gratified through propaganda work, no matter how successful it might be in the long run. In a particularly felicitous turn of phrase, characteristic of the quality of the prose throughout the book, Hardy comments that terrorism offered the radicals "an opportunity for action, a chance for heroism, [and] an exaltation of self" (p. 162), which she rightly considers especially important in a movement so dynamized by the impatience of youth. Once the decision to engage in terrorism had been made, terrorist operations began to consume a greater and greater share of the resources, time, and energy of the organization, leading through an incremental escalation of violence to the momentous decision to assassinate the tsar.

Hardy's book is a model of a successful historical monograph: clearly written, exhaustively researched, cogently argued, a convincing case for the thesis the author presents. The reader will finish the book with greater knowledge and understanding of the revolutionaries of *Zemlia i Volia* and of the psychology that shaped their political choices. To the extent that revolutionaries in different historical circumstances have often confronted these same choices, *Land and Freedom* is a work from which historians of different areas of specialization should profit greatly.

JAY BERGMAN
Albright College

I.M. IGNATENKO, *Fevral'skaia burzhuzno-demokraticheskaia revoliutsiia v Belorussii* [The February Bourgeois-Democratic Revolution in Belorussia]. Minsk: Nauka i Tekhnika, 1986. Pp. 343. 2 r. 20 k.

Belorussia presents an interesting case study for examining the spread of the Russian revolution to the provinces. Overwhelmingly an agrarian region, Belorussia contained the cities of Minsk, Vitebsk, Mogilev, and Gomel' with small contin-

gents of the proletariat. It also belonged to the western front directly facing the Germans, who had already occupied a part of its territory. The February revolution touched off in Belorussia all of the four mass movements that became the major ingredients of the Russian revolution of 1917: the movements of workers, peasants, soldiers, and national minorities.

I. M. Ignatenko's work is a welcome addition to the growing literature, both in and outside of the Soviet Union, on the revolutionary process in the provinces. Discussing the revolution in the cities, the army, the villages, and the Belorussian national movements in March–April, he traces the process by which indigenous revolutionary movements sprang up with the creation of new organizations, which already by the end of April had begun to reflect the profound social chasms in society.

He sees this process basically through the narrow, partisan prism of Soviet historiography. Thus we are exposed to a heavy dose of stereotyped characterizations of the liberals (determined to restore the old order), the socialist compromisers (always eager to support the bourgeoisie), and the Bolsheviks (united by Lenin's correct policy, and serving as the sole locomotive of the revolution). If we have the patience to see beyond these hackneyed interpretations, however, this book offers a wealth of valuable information, sometimes derived from hitherto unused archival materials and published materials, notably contemporary local newspapers.

We learn, for instance, the detailed process by which two centers of power, the *obshchestvennye* committees of public safety and the soviets of workers' and soldiers' deputies, were simultaneously formed. There are excellent descriptions of local variations on the relationships between the two. The Minsk Soviet accommodated itself more willingly with the bourgeois power, actively joining its own executive committee, while the Vitebsk Soviet took a more hostile attitude toward the bourgeois local power. Organizing the militia became a crucial issue, over which the contest of the two powers began to be waged. Here again, differences existed between Minsk and Gomel' in the degree to which the soviets controlled the militia organizations. As for the army, Ignatenko provides additional information on, among others, the First Congress of the army representatives in the western front and on the revolutionary process within the army already masterfully presented by Allan Wildman.

The author is most successful in presenting materials on the national movements, divided between the bourgeois-oriented Belorussian National Committee (BNK) and the more radical

Belorussian Socialist Gromada. Maintaining generally a critical attitude toward the BNK, he nevertheless deals with the subject with sensitivity: he treats, for instance, K. K. Kastovitskii, one of the leaders of the BNK, with sympathy. Less successful is his treatment of the revolution in the countryside, although here, too, one can also pick up valuable pieces of information on the growing split between *volost'* committees and peasants' committees at the village level.

Curiously, the most obscure part of Ignatenko's work is the description of the Bolshevik party. M. V. Frunze figures prominently, but his activities and pronouncements are presented without any reference to the general development of the Bolshevik party organizations in Belorussia. As to the Belorussian Bolsheviks' attitude toward the issues that divided the party in March and April, such as the policy toward power and the attitude toward other radical socialist organizations, Ignatenko has nothing to say.

Ignatenko's book represents a typical example of a good Soviet historical work produced by a competent historian. He provides us with valuable new materials, but he remains bound by an ideological straitjacket and unable to construct the new interpretation that these new materials deserve. How this is going to change under *glasnost'* remains to be seen.

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A. P. REENT. *Rabochii klass Sovetskoi Ukrainy na zavershaiushchem etape grazhdanskoi voiny* [The Working Class of the Soviet Ukraine in the Final Stages of the Civil War]. Kiev: Naukova Dumka. 1984. Pp. 166. 1 r. 50 k.

Between November 1919 and January 1920, the White forces of General Denikin were swept from the Ukraine by the Red Army, but on April 25 the Polish army advanced into the western Ukraine, and in June Wrangel's forces pushed north from the Crimea. By November, the Bolsheviks had triumphed over both and were in complete control of the Ukraine. One receives but a dim sense from this book of the military events that formed the backdrop to the establishment of Soviet power in 1920, yet a review of the military actions casts doubt on the book's central thesis that it was the working class that played the "leading role" in this process. Certainly, the industrial workers of the Donbas, Khar'kov, or Krivoi Rog were among the keenest supporters of the Bolsheviks (many of them, incidentally, of Russian, not Ukrainian, extraction, a fact not mentioned in the text), but the real credit must surely go to the Red Army itself.

The unthinking repetition of shibboleths about the "leading role" of the working class is typical of this book, which, despite an apparently solid foundation in archival research, has little to say that is original. It has a few interesting things to say on the measures that were taken to revive the battered economy, recounting the heroic efforts of miners and metallurgical workers to secure supplies of fuel and food, of railway workers to combat transport chaos, and the small but poignant attempts to establish a rapport with the peasantry (to whose general disaffection no reference is made) by setting up repair depots in the countryside. The Bolsheviks proceeded to enforce the panoply of War Communism, and one infers that there was not insignificant resistance, especially to the introduction of one-person management and to the reduction of the trade unions. Allusion is made to the strength of Menshevism, although nothing is said about the Workers' Opposition. The book, then, is not without value, but it falls short of the scholarly standard set by E. G. Gimpel'son and D. A. Baevskii, the best Soviet historians of labor during the civil war.

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EARL F. ZIEMKE and MAGNA E. BAUER. *Moscow to Stalingrad: Decision in the East*. (Army Historical Series.) Washington, D.C.: Center of Military History, United States Army. 1987. Pp. xv, 558. \$24.00.

This superb study, long on hard facts and mercifully short on interpretation, is the best introduction yet published in English to the first year and a half of the titanic struggle known in the USSR as the Great Patriotic War. Using Soviet and German resources, the authors have assembled concise but informative accounts of each major battle fought between June 22, 1941, when the German forces crossed into the USSR, and February 2, 1943, when the last Germans surrendered in Stalingrad. Not only are there accounts of battles but also a wealth of information about the Soviet and German actors who played in this drama of the greatest land battle ever fought. Although the authors obviously have their own views, the conclusions that can be drawn are largely left to the reader; this reviewer draws the following inference: during most of the period under question, the Soviets enjoyed both quantitative and qualitative superiority and yet managed, by the sheer incompetence of Joseph Stalin and most of his commanders, to squander numerous opportunities to inflict a decisive defeat on the enemy. It seems incredible that in the early days of the war

the Germans advanced faster against the Russians than they had against the hapless Poles in 1939. What saved the day was the bravery and fanaticism of the Russian troops who continued to fight and bleed the enemy, whose losses could not be replaced. This sacrifice seems almost inexplicable in view of the regime for which the Soviet soldiers were fighting. The tide finally turned in Stalingrad not because of Soviet generalship but because Adolf Hitler could not decide on his strategic goals; in addition, the "permanently operating forces" were on the Soviet side. The military epitaph for this period, however, was best expressed by General Charles de Gaulle, who was less impressed by the eventual Soviet victory than the depth of the earlier German advance.

Comparison between this book and John Erickson's *Road to Stalingrad* (1975) is inevitable. Erickson's encyclopedic coverage is primarily for specialists, encompassing not only history but also a considerable amount of personal interpretation. The present book's goals are more modest and confined primarily to detailing actual events. Unlike Erickson's book, the present volume is not at all intimidating, and it exhibits superior editing and indexing. In the final analysis, both books have a place in appropriate libraries. My sole reservation about the book under review is that, in their admirable attempts at objectivity, the authors do not give a completely accurate picture of the desperate situation the Soviets faced and the improvised nature of many of their decisions, an impression that this reviewer gets from reading the more realistic memoirs written during the Khrushchev years and from two Soviet historical "novels" written along the lines of *War and Peace*: A. Chakovskii's *Blokada* (1969), based on the unpublished memoirs of Marshal Timoshenko, and I. Stadniuk's *Voima* (1972), which somehow did not make it into the present book's extensive bibliography. Highly recommended.

MICHAEL PARRISH
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MARTIN MCCAULEY, editor. *Khrushchev and Khrushchevism*. Bloomington: Indiana University Press. 1987. Pp. xii, 243. Cloth \$29.95, paper \$10.95.

This volume is the product of a conference held in March 1985 at the University of London. The purpose of the conference was to assess Nikita Khrushchev's contributions to Soviet historical development, analyze the nature of the phenomenon "Khrushchevism," and thereby gain a deeper appreciation of the systemic problems facing Khrushchev's successors and Mikhail Gorbachev. Yet, according to the editor, "[t]he object of this

volume is to provide an overview of the Khrushchev period for the general reader" (p. 2). The result of this ambiguity of purpose is a volume that is very uneven in quality and coverage as well as somewhat schizophrenic in mission.

The contributors, for the most part, know their fields well, and the number of issues and areas covered is unusually broad. Martin McCauley begins with an analysis of Khrushchev's qualities and strategies as a leader. Graeme Gill analyzes Khrushchev's contribution to the institutionalization of the Soviet political process. Ronald J. Hill summarizes the Soviet leader's innovations in the areas of political organization and doctrinal change. Alec Nove follows with a short analysis of the problem of industrial organization and why Khrushchev's reforms in this area quickly failed. M. J. Berry provides a detailed and informative survey of the state of science and technology under Khrushchev and the reasons why many of his policies failed to improve the diffusion of innovation. G. A. E. Smith then surveys the agricultural scene, D. Filtzer analyzes labor policy, and Alastair McCauley surveys social policy. The volume then turns to aspects of foreign policy, which is far less thoroughly and satisfactorily covered than is domestic policy. Michael Shafir provides a penetrating analysis of Khrushchev's policies toward Eastern Europe; Harry Hanak all too briefly interprets the general trends of Khrushchev's foreign policy; Christoph Bluth analyzes defense policy; and Frank K. Roberts, who was British ambassador to Moscow during 1960-62, describes his personal encounters with Khrushchev, thereby focusing primarily on the Soviet leader's personality. The book ends with a useful bibliography but no conclusion.

The general reader will probably learn more than he or she wishes about the state of Soviet internal affairs, and Khrushchev's policies with respect to them, by reading this volume. Specialists on the Soviet Union will not learn much that they have not already absorbed from other works, except in those highly specialized areas to which they have not devoted the time. Specialists will also be disappointed to discover that the volume fails to make use of an opportunity to advance scholarly thought about how to evaluate Khrushchev's distinctive contribution to Soviet history or how to define the difference between Khrushchev and "Khrushchevism."

McCauley attempts to make an advance on the first of these questions by introducing a useful distinction between traits of leadership and analyzing Khrushchev as a transitory, transitional, or original leader. He develops the idea to a limited extent in chapter 1 but cannot do justice to it without a concluding chapter that draws on the

material in subsequent chapters. Such a conclusion is not forthcoming. What's more, very few of the authors chose to take up the editor's challenge. Only Shafir and Berry did so, and the results are discussions of unusual analytic depth and richness. Most of the contributors also avoided the challenging task of determining the extent to which Khrushchev himself was responsible for the policies enacted. Yet, without such an effort, it is difficult to see how one can evaluate Khrushchev's contribution to Soviet history, much less define the content of something called "Khrushchevism."

The enterprise would also have been enriched by relating many of the arguments and interpretations to controversies extant in the preexisting literature on Khrushchev. The reader is left with little awareness of contending positions or even of some of the important work already done on these questions.

Many of the chapters end with brief interpretations of what did or did not change under Leonid Brezhnev. Specialists will wonder about the purpose of such addenda.

Finally, the volume has been poorly edited. In the first half of the book, the reader will suffer through four summaries of the particulars of the *sovnarkhoz* reform of 1957. And, throughout the volume, the reader will encounter annoying typographical errors, a few of which confuse history.

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NEAR EAST

NEHEMIA LEVTZION and JOHN O. VOLL, editors.
Eighteenth-Century Renewal and Reform in Islam. Syracuse, N.Y.: Syracuse University Press. 1987. Pp. 200. \$24.95.

Reading between the lines of the introduction or, rather, re-reading the introduction after the rest of the book, it is apparent that, within his chosen field, Nehemia Levtzion has had a hard row to hoe. *Eighteenth-Century Renewal and Reform* bears the stamp of his comparative approach to the history of the Muslim world from a well-prepared position in West Africa; like the previous volume for which he was responsible on the subject of Islamization, it is the product of a seminar he organized to concentrate the attention of scholarship upon a general problem through a series of regional studies. The problem is that of Islamic reform in the eighteenth century, much vexed in West Africa, where a direct connection between the *jihād* of Usman dan Fodio and the Wahhābi

movement in Arabia has long seemed improbable, despite the coincidence. It is the common background, and the common features, of such movements that the book sets out to investigate, beginning with a masterly summary of the question in West Africa that, even more than the introduction, offers a prospectus for the whole. The gentle scholarship of Louis Brenner on the subject of Usman dan Fodio, however, promptly provides a quiet but clear warning of the difficulties of seeing a truly great man as the sum of his parts, and the following piece, by John O. Voll, a more fundamental illustration of the obstacles in the way of the program. Voll is Levzion's fellow editor, and his plotting of master-pupil relationships across the world of scholarly and saintly Islam in the eighteenth century offers, according to the preface, a way into its ramifications and various concerns. But his description of the Mizjaji family in Yemen is disappointing, not simply because the Mizjajis were so very obscure. Their obscurity, I take it, is the point: they helped maintain and reproduce the vast network of the intellectuals that, on the one hand, preserved the ideals of Islam and, on the other, gave rise to the reformers and revivalists. But such a network had been in place, *mutatis mutandis*, for centuries, performing just such a function for the individuals of each generation who came to some new understanding of the faith. The tracing of scholarly pedigrees, itself a preoccupation of the scholarship in question, will not of itself answer why, or even how, such understandings came about. In this case, there seems to have been "a special combination of *hadith* studies and social reform-motivated *tariqah* affiliation" (p. 88). But a study of the Mizjajis of this particular world hardly demonstrates its significance—or theirs.

What should have been the centerpiece of the collection thus hardly serves its purpose. The remaining articles are well written, each in its own way, against the theme. Rudolph Peters tells the brief tale of a radical preacher in Cairo who challenged the religious establishment on scriptural grounds. But he was a Turk, whose fundamentalism reflected the discontent of the unemployed graduates of the schools of Istanbul; he provoked a brief riot of the Janissaries but elicited no sympathy from the Egyptians. Frederick de Jong very carefully explains why the common attribution of a reformist revival of the Khalwatiyya to Mustafa Kamal al-Din al-Bakri (d. 1749) is mistaken. Etan Kohlberg's "Aspects of Akhbari Thought" adds yet another little masterpiece to his historical studies of Shiite theology; but the subject, of course, remains Shiite, and its wider significance is not discussed. Richard Eaton, finally, relates the spread of Islam in Bengal to the

clearing of the forest for rice cultivation. The picture is wholly convincing but is scarcely one of "renewal and reform." Turning back to the introduction, then, we see the necessity for the remark that "understanding the limits of the pattern is . . . as important as understanding the positive definition" (p. 16). Had the editors been able to include "two excellent papers by Anthony Johns and Marc Gaborieau, which throw new light on renewal and reform in eighteenth-century Indonesia and India" (preface), the string, as it were, might have been stronger. Perhaps not.

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MATTI MOOSA. *Extremist Shiites: The Ghulāt Sects*. (Contemporary Issues in the Middle East.) Syracuse, N.Y.: Syracuse University Press. 1988. Pp. xxiii, 580. \$37.50.

This book examines the history, beliefs, and rituals of heterodox Muslim groups in Iraq, Iran, Syria, and Turkey who deify Ali ibn Abi Talib, the cousin and son-in-law of the Prophet Muhammad. Although the majority Sunni Muslim community classifies these sects as "extremist" (in Arabic, *ghulāt*), this adjective refers to their theologies and not to their political roles. Most Muslims, of course, reject the divinity of any human being, and honor Muhammad as their prophet and Ali as one of his merely human successors. Few of the groups Matti Moosa examines have mobilized for political action on the basis of religion, in contrast to the more scripturalist Twelver Shiites of Iran and Lebanon.

Moosa's book is an accessible and useful summary of what is known about these heterodox Shiites. He looks first at Turkish or Turkoman sects, such as the Shabak in northern Iraq, and in Turkey at the Bektashi Sufis and the Qizilbash (or Alevis). He argues that these groups are survivals of the religion practiced by the followers of the Sufi-Shi'i order of the Safavis, who conquered Iran in 1501. In converting Iran to Shiism, the Safavid dynasty moved toward a more orthodox and scripturalist form of Twelver Shiism, whereas the isolated Turkomans of Iraq and Anatolia clung to the esoteric doctrines of their folk religion.

Other major heterodox groups examined include the Ahl-i Haqq of Iran, which might legitimately be seen as a post-Islamic religion that has had several divine prophets since the seventh century but which recognizes Ali as one of its holy figures. Finally, Moosa dwells at length on the Arabic-speaking Nusayris of Syria, especially im-

portant because they have supplied the contemporary political elite of that country. Nusayris recognize a trinity of Ali, Muhammad, and the disciple Salman the Persian, and also believe in Twelve Imams. Like many of the heterodox Shiites, they accept metempsychosis (a doctrine most Muslims reject).

The groups Moosa examines are of interest to students of religion, and some are demographically and politically important. The Qizilbash and Bektashis in Turkey may make up 20 percent of the Muslim population, and, although the Nusayris are only about 10 percent of Syrian Muslims, they are in control of the government there. The other sects are much smaller.

Moosa's survey has an almost encyclopedic quality and seldom goes beyond a descriptive approach. It is sometimes poorly organized. Although the author no doubt ably summarizes many medieval manuscripts relating to these groups that have found their way into Western repositories, he is less reliable when talking of contemporary events. He often accepts simplistic conspiracy theories, especially about the Nusayris, and his book should be used with caution for approaching issues in the modern Middle East.

JUAN R. I. COLE
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YOSEF GORNY. *Zionism and the Arabs, 1882–1948: A Study of Ideology*. Translated by CHAYA GALAI. New York: Clarendon Press of Oxford University Press. 1987. Pp. x, 342. \$67.00.

The combined motivation of seeking refuge from persecution and desiring the philosophical return to *Eretz-Yisrael* (Palestine, the Holy Land) gave birth to modern Zionism. The goal of nineteenth-century Zionist thinkers was the reestablishment of a Jewish presence on a national territory. Since modern Zionism's intellectual origins were found in the Jewish diaspora, it is not surprising that both the methods of achieving the goal and the assessment of the obstacles were equally divergent. As an ideology, Zionism was pluralistic and dynamic. For adherents and antagonists alike, its source of strength was its territorial and demographic growth.

From modern Zionism's earliest beginnings to the divisions within Israel's national unity government in 1988, the variety of Zionist attitudes and actions has come from amalgams of three key variables: articulation and implementation of the preferred social and economic orders; the worldwide status of the Jewish physical condition; and the strengths and weaknesses of the Arab community of Palestine and of neighboring Arab states.

In this excellent book, Yosef Gorny presents clear and probing analyses of the origins and permutations of Zionist ideology toward the Arabs from 1882 until the establishment of the state of Israel in 1948. Gorny synthesizes the continuity and change of Zionist philosophical development within a broad spectrum and succession of diverse Zionist leaders. Originally published in Hebrew, the book is divided into three major time segments corresponding to the Zionist presence in Palestine and the context of world events affecting the Jewish condition: 1882–1917, 1918–29, and 1930–48.

The author focuses on one of Zionism's unprecedented contributions to Jewish history. "For the first time, as a result of the Zionist settlement enterprise," writes Gorny, "Jews were shaping the destiny of another people. . . . This was a new experience for the Jewish people and one of the more unsettling aspects of national renaissance" (p. 321). For some Zionist thinkers, the Arabs in Palestine were an unwanted problem that was to be ignored or denied as Jews sought to establish a majority presence in Palestine; other Zionists sought either partial or complete accommodation with the Arab population, with solutions ranging from the creation of a bi-national state to association through class consciousness.

Gorny's rigorous research and mining of the sources are comparable to the excellent scholarship of Neil Caplan, Walter Laqueur, Neville Mandel, and David Vital. Like these authors, Gorny has enriched our historical understanding of Zionism's relationship with the Arabs of Palestine.

KENNETH W. STEIN
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AFRICA

JAN HOGENDORN and MARION JOHNSON. *The Shell Money of the Slave Trade*. (African Studies Series, number 49.) New York: Cambridge University Press. 1986. Pp. xv, 230. \$34.50.

Marion Johnson, who died on February 12, 1988, has published pioneering and stimulating articles on West African economic history during the last two decades. Johnson also regularly presented papers at African Studies Meetings and International Economic History Conferences. This is Johnson's first book and, alas, her last. Her co-author, Jan Hogendorn, is a well-known and respected economic historian whose work is known to the readers of the *AHR*. Together, Johnson and Hogendorn have written an interesting, comprehensive, and lucid history of *The Shell Money of the Slave Trade*.

Johnson's articles in the *Journal of African History* in 1970 provided the impetus for the present work. Hogendorn's contribution stems from his interest in the relation between African indigenous monies and the slave trade. The book, which is the result of archival work and some field work in Europe, Africa, and Asia, is a detailed and comprehensive history of the cowrie shell. The book exhaustively examines cowrie shells' geographical and chronological formats. It looks at the demand and supply of cowries, their origin and location in Africa, the methods of counting, and the eventual decline and demonetization of the shell as currency.

Cowrie shells, most of them originating off the Maldive Islands of the Indian Ocean, once came to West Africa through North Africa and across the Sahara Desert. Later, the shells were used by European trading company representatives, who carried them to Lisbon, Amsterdam, or London as ballast and then reshipped them to West Africa. The shells were used as currency before, during, and after the slave trade. In fact, when Europeans made contact with West Africa during the fifteenth century, cowries were in wide use as money. In West Africa, the cowries became the shell money of the slave trade when sellers of slaves demanded cowries for their wares. The expansion of the African trade and the increasing connection of the cowrie shells with slave exports from Africa led to steadily rising demands. The slave trade was without question a major reason that cowrie imports reached record levels during the eighteenth century. Europeans who imported the shells to Africa would not take them back once paid to African traders.

Near the end of their life in the nineteenth century, cowries suffered a hyper-inflation, which affected their use and circulation. In West Africa, shell money could have enjoyed a long and important life as a subsidiary currency for small purchases. However, because of a combination of opposition by the colonial authorities and continuing twentieth-century inflation, that was not to be.

This fascinating, lucid, and clear account is the kind of work one wishes one could have written. Hogendorn and Johnson have written a valuable book. It is sad that this has to be their last joint venture.

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KEN SWINDELL. *Farm Labour*. (African Society Today.) New York: Cambridge University Press, 1985. Pp. vii, 204. Cloth \$29.95, paper \$10.95.

There is much to ponder in this excellent short volume, its subject at once more limited (African farm labor, not farm labor generally) and more provocative and historical than the title indicates. Ken Swindell analyzes the role of African farmers and farm workers within the contexts of both traditional agricultural units and modern export crop systems that have been fully absorbed into the capitalist sector.

Agricultural history in Africa embraces an infinitely complex and bewildering set of facts, socio-economic structures, and issues, and to generalize about the response of farm workers to economic change throughout the whole of sub-Saharan Africa is a daunting task. Yet the author manages, in a mere two hundred pages, to cover the major geographic regions and to include scores of case studies on particular ethnic groups. Although Swindell follows no single ideological line, he does use a vaguely Marxist conceptual framework to provide a central developmental (and historical) thrust for what might otherwise appear as an unintelligible mishmash. In the author's words, "The difficulty in understanding rural society is the one encountered throughout the social sciences: that of reconciling individual actions and perceptions with the larger structural forces of society and state, and the manner in which these contradictions have developed historically." Thus, the reader will find familiar Marxist concepts sprinkled throughout the volume, but these are useful correctives to the numerous textbooks, written by farm scientists and economists, which often do not include historical and sociostructural issues at all. To flesh out his chapters, Swindell draws on monographs from many countries in West, East, and Central Africa plus the fruits of his own field research conducted in Gambia and northern Nigeria.

Swindell's approach is sophisticated yet realistic. He avoids a bias still all too common in conventional agricultural development handbooks, that is, speaking of African rural economies as static units incapable of change without foreign assistance. He also avoids the opposite tendency to idealize African "peasants" (he does not shy away from the term) as undifferentiated "economic men" ready for instantaneous responses to the development directives of extension experts or to fluctuations in export crop prices. At the core of his analysis lies an awareness that African rural economies include a variety of types that are constantly undergoing change, and, hence, it is naive to write of African farmers as if they were homogeneous aggregates making predictable responses to economic stimuli. A major flaw in externally managed agricultural aid packages of the past has been the unwillingness of soil sci-

tists and plant breeders to slog through necessary research on local cultures and social organization as well as indigenous economics to ensure that development programs are "location-specific."

As a shortcut to understanding economic change in Africa, Swindell discusses three basic agricultural systems that can exist simultaneously: traditional production systems in which custom and kinship still govern labor organization; fully developed commercial crop zones where market relations are dominant and where a mobile agricultural labor force is organized around wage payments; and transitional economies that may exist on the peripheries of commercial crop zones. In transitional economies, the same workers may grow cash crops on large estates, grow subsistence food crops on small plots, yet also sell their services as migrant wage laborers. In Swindell's view, government planners and foreign aid experts must pay particular attention to the perceptions of the migrant laborers to avoid many of the policy mistakes of the past.

Swindell also evaluates some of the development programs applied with uneven success in conservative-capitalist nations (Ivory Coast, Nigeria, Kenya) as well as in socialist countries (Tanzania, Guinea, Mozambique). In an insightful conclusion, he notes a common flaw in both Western-oriented, capitalistic programs and state socialist schemes. Planners often create mental blocks by grafting the ideology and administrative paraphernalia of the modern state on to localized societies still strongly shaped by criteria of age, sex, and kinship and so "less than willing to meet national plans and objectives."

Only one or two shortcomings need be noted. The bibliography is not long enough, and footnote references are rather thin. Much of Swindell's evidence comes from West African sources. East Africa receives satisfactory treatment, but Central Africa gets only slight attention and Southern Africa practically none at all. In highlighting recent research on agrarian change, Swindell most frequently cites work by British scholars. More references to recent field research in agricultural economics, as well as in history and anthropology, undertaken at African, American, and French universities would have strengthened documentation on locally inspired innovation and on the quantitative results of state-sponsored district development schemes.

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LANDEG WHITE. *Magomero: Portrait of an African Village*. New York: Cambridge University Press. 1987. Pp. xii, 271. \$29.95.

Landeg White's elegant book is something of a minor masterpiece and may well influence writing about Africa more than its size, or the importance of its subject, would at first suggest. It is a study not so much of a single African village as of a small area of the Malawi countryside where a succession of village communities have lived and worked over the last one hundred and thirty years. It is, of course, a study in depth, but the evolving economy of the Shire Highlands and the complexities of the *thangata* labor system that dominated the lives of the much-maligned *Anguru* immigrants have all been woven into a fascinating narrative punctuated by the sudden eruptions of important historical events. All this could have been done in a pedestrian way, the reader trudging through pages of sociological jargon or getting lost in the welter of meaningless names that so often marks works based on oral tradition, but this book is a model of how to treat weighty matters with a lightness of touch and a readability that are wholly admirable.

Magomero is first heard of in history when Bishop Charles Mackenzie and the Universities Mission to Central Africa (UMCA) arrived in the Shire Highlands in 1861 to try to establish their first mission among a population that was facing Ngoni raids and the expansion of the Yao slavers from the east. The UMCA mission stood for two years like a rock around which these events eddied, before death eliminated so many of the missionaries that it had to be withdrawn. The author's delight in the ironies of history appears in his epitaph on the mission. "The missionaries declared they had come to teach the people agriculture. But they had cleared land in the middle of the dry season. They told everyone to grow cotton, but cotton was a valley crop, unproductive in the Highlands. They lectured against slavery, yet had assembled the largest group of refugees in the whole region" (p. 35). In spite of the UMCA, the Yao established themselves in the area only to find that the colonial government granted huge tracts of the highlands, including Magomero, to plantation companies, which then began to develop the capitalist economy. A succession of Scottish planters complemented the work of the Scottish missionaries and pioneered coffee, cotton, tobacco, and tea, each crop having its heyday before suffering downturns in the market and giving way to the next experiment. Meanwhile, people were migrating into the area from Mozambique, and these newly arrived *Anguru* were willing to settle under Yao headmen or on the plantations under a system of tenure called *thangata*, which was in practice a kind of serfdom. The book shows the fatalism that settled alike on plantation owners and *thangata* peasants, both of them locked into a

system that allowed them no escape and no real prospect of progress or change. The plantation owners labored under huge debts and could only succeed by extracting labor at excessively cheap rates or by not paying wages at all. The *thangata* laborers for their part had no prospect of bettering themselves and no escape from their labor tenancies, risking losing their land if they gave up working.

Into this world came John Chilembwe with his Providence Industrial Mission, and White shows how the men of the region were caught up in the prophecies and expectations of his church. The Chilembwe rising was a man's event. Five years after Chilembwe's death, the memory of his rising was already "so far removed in its explanations and emphases from ordinary accounts of the rising as to challenge the historian's most basic assumptions" (p. 155), and White concludes that today "very few incidents of the Chilembwe rising can be learned by questioning the older people in the villages" (p. 156). Oral tradition is exceptionally treacherous ground, particularly among communities such as the *Anguru* where land is "owned" and worked, and cultural continuity is provided, by women but political action takes place in the world of men. The Scottish planters were overtaken by world depression and by the rising prosperity of peasant tobacco growers. White relishes the irony of the planters, who, having once argued that they were bringing development and civilization to Africa, were forced to argue that the African peasant and the colonial authorities owed them a living for having risked so much capital and effort in Africa.

As the historical life of the community reaches the present day and White, living in the community, becomes part of its experience and himself helps form the life he is recording, the book's theme slides gently into autobiography. "Towards the end of the service," (the author was attending Sunday service in a church built on the site of Bishop Mackenzie's mission church), "I was formally welcomed . . . Why was I here today? . . . Why was I present? . . . It was a fair question. The true answer was that I was looking for an ending for the book I was going to write about them" (p. 258).

If in writing, style and form count for as much as content, then this book deserves to be read for the elegance and subtlety with which it encapsulates the whole history of colonial Nyasaland in the life of a single community. It is a perfect example of how to explain great issues by looking at their effect on the lives of little people.

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ASIA AND THE EAST

TETSUO NAJITA. *Visions of Virtue in Tokugawa Japan: The Kaitokudō Merchant Academy of Osaka*. Chicago: University of Chicago Press. 1987. Pp. x, 334. Cloth \$37.50, paper \$14.95.

Tetsuo Najita is among the most important students of Tokugawa intellectual history in the West. His seminal articles and books on neo-Confucian thought are among the most persuasive works in the field. This study provides an intellectual biography of the major figures associated with the Kaitokudō merchant academy of Osaka. It is not a history of the institution but rather a history of the major thinkers identified with it during its heyday from 1728 to 1821. The academy provided the locus of intellectual activity as well as the incentive for Najita's subjects to focus their concerns on issues of ethics and morality in Osaka merchant society. By tracing the thematic development of merchant scholarship and relating it to the broader community of intellectual life in Tokugawa Japan, Najita illustrates how intellectual life transcended class and regional boundaries. The Kaitokudō provided not only a place for scholarship but also a meeting ground where local scholars and students and travelers from other parts of the country could meet for seminars and intellectual exchange without fear of restraint or censorship. As a *Bakufu*-chartered academy, it offered a protected venue for intellectual debate. Its charter defined and limited its functions while providing the stimulus for philosophical inquiries into the ethical concerns of its merchant participants.

The intellectual traditions associated with the Kaitokudō were separate from and at odds with the *Shingaku* tradition of merchant thought associated with Ishida Baigan. Kaitokudō thinkers took exception to *Shingaku* as a spiritual system for merchants and looked within the Confucian tradition for justifications for the activities of the Osaka merchants. The focus of Kaitokudō thought was "the meaning of virtue" (p. 2). As a school it became the scholarly center of intellectual life in west central Japan.

Among the most important scholars identified with the Kaitokudō were its founders, Miyake Sekian (1665–1730) and Nakai Shuan (1693–1758), and their intellectual successors, Tominaga Nakamoto (1715–1746), Goi Ranju (1697–1762), Nakai Chikuzan (1730–1804) and Nakai's brother Riken (1732–1817), Kusama Naokata (1753–1831), and Yamagata Banto (1748–1821). Najita provides intellectual biographies of each of these individuals and relates their scholarship to the Tokugawa intellectual community and Osaka

merchant society. Consequently, one gets both a detailed analysis of the development of Kaitokudō thought and its associations with the wider world of intellectual life in Tokugawa Japan.

Basic to Kaitokudō thought were merchants' concerns with issues of political economy, which resulted in what Najita defines as a "complex discursive interaction between a 'political' view of economics and an 'economic' view of politics in which merchants . . . played a key role" (p. 8). As Najita illustrates in his analysis, "merchants developed an ideology that justified their acting economically in the public realm, thereby rendering their analysis and insights into the plight of the economy as being political ones. The intellectual history of the Kaitokudō clearly reveals this dynamic line of development" (p. 9). Najita concludes his analysis with a focus on Yamagata Banto's *In Place of Dreams* (*Yume no shiro*, which was drafted between 1800 and 1805) and illustrates how this work synthesizes the ideas taught and discussed at the Kaitokudō, particularly the questions of virtue and the ethic of trust and righteousness.

In an epilogue Najita addresses the interpretive premises that underlie this study. For him, "Tokugawa thinking on political economy . . . contained the dynamic capacity to cross class lines and travel outside of the ruling aristocracy to assume an ideological significance among Osaka merchants" (p. 285). Tokugawa conceptions of political economy changed as thinkers directed their attention to how knowledge instructed human action in everyday life. Merchants were better prepared than the samurai aristocrats to analyze the structure and content of political economy. Thus, "the polity itself was a field of knowledge appropriately within the systematic purview of merchants" (p. 287). Based on a close reading of both primary sources and secondary works, this important book illustrates the highest quality of contemporary scholarship. The result is a major enhancement of our understanding of the role of merchant contributions to the intellectual history of Tokugawa Japan.

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ANN BOWMAN JANNETTA. *Epidemics and Mortality in Early Modern Japan*. Princeton: Princeton University Press. 1987. Pp. xxii, 224. \$30.00.

This book is the first Western work on the impact of epidemics on Tokugawa mortality and a major contribution to the growing literature on the historical demography of early modern Japan. The causes of Tokugawa Japan's stationary population

after about 1720 are related to the broader questions of the state's level of economic development. The Marxists emphasize Malthusian checks to population growth: although war can be ruled out as a factor during Pax Tokugawa, famines and pestilence brought about periodic mortality crises. The early modern development model (as Ann Bowman Jannetta calls it), espoused by Thomas C. Smith and Susan B. Hanley among others, argues that both fertility and mortality were low and that population size was limited by positive checks such as late marriage and infanticide, not as a response to poverty but as a form of family planning.

Jannetta makes imaginative and impressive use of a rich variety of sources: Buddhist temple death registers (*kakochō*), hitherto little used by Western historical demographers; research of Japanese medical historians such as Fujikawa Yū; contemporary descriptive accounts; records of celebrations for the recovery of Tokugawa family members from smallpox, measles, and chicken pox (*sasuyu*); woodblock prints. Her research reveals that bubonic plague and typhus were conspicuously absent in Tokugawa Japan and that, while there were epidemics of smallpox, measles, dysentery, and cholera, only smallpox was a major killer, and even its effects were not as severe as in early modern Europe. Jannetta suggests that "Japan's geography and her isolation from the major world trade routes provided a *cordon sanitaire* that prevented major diseases from penetrating Japan until the mid-nineteenth century" (p. 5). "The fact that Japan was spared the legendary epidemics of late medieval and early modern Europe helps to explain Tokugawa Japan's remarkably large population, and perhaps the need for strong preventive checks to population growth" (p. 207). Her conclusions thus support the early modern development model's picture of low fertility and low mortality.

Like all good studies, Jannetta's book inevitably raises as many questions as it answers. She has relied heavily on the Buddhist temple death registers and local histories of two regions, Sendai on the northeastern coast and Hida in the mountainous interior, which were chosen to represent two extremes in terms of accessibility to the outside world, and has convincingly demonstrated the relative unimportance of acute infectious diseases to mortality. Still, given the possibility of wide regional variations in mortality patterns, more death registers from different areas must be analyzed before her thesis of the comparative insignificance of epidemics can be generalized for all of Japan with complete confidence. In addition, the impact of chronic infections or nutrition-deficient diseases, and famines that supposedly killed as much as one-fifth to one-half of certain regional

populations, must also be considered in future studies of mortality. In accepting Ronald Toby's rejection of the myth of Tokugawa Japan as a closed country, Jannetta had to qualify her isolationist thesis by adding state policy and social customs as explanatory variables for Japan's relative immunity from epidemics despite considerable contacts with East Asian neighbors. She conjectures that the Japanese government may have actively kept foreign diseases out of the country by quarantining ships with infected persons aboard. Also, the Japanese may have had higher standards of public health and personal hygiene and healthier eating habits than Europeans: the removal of human excrement from cities to the countryside for use as night soil, the boiling of water, the consumption of cooked food, and religious taboos against meat eating. These conjectures are plausible but require documentary substantiation. Jannetta's admirable work thus points to a need to place Japan's unique demographic regime in cultural and political context.

ROBERT Y. ENG
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PATRICIA BUCKLEY EBREY, editor. *Family and Property in Sung China: Yüan Ts'ai's Precepts for Social Life*. (Princeton Library of Asian Translations.) Princeton: Princeton University Press, 1984. Pp. x, 367. \$37.50.

Family and Property in Sung China is a translation of the *Shih-fan* (Precepts for Social Life) written in 1178 by Yüan Ts'ai (flourished 1140–95) of the Sung dynasty (960–1279). The *Shih-fan* is a manual of advice addressed to family heads on the subject of how to direct and manage their families' moral, social, and material life. Its original Chinese text comprises three chapters: "Getting along with Relatives," "Improving Personal Conduct," and "Managing Family Affairs" in Ebrey's rendering. Patricia Ebrey's translation of the book is complete and includes essential annotations, and she also provides an extensive introductory essay in which she not only presents the background information on Yüan Ts'ai and the book *Shih-fan* but also addresses general questions about the upper class of traditional China such as the family as an institution, social life, material life, the value system, and so forth.

The importance and value of Ebrey's book must be assessed in three different perspectives. First, in the current state of American scholarship on China, this is a welcome contribution to the study and understanding of traditional Chinese society. Yüan's book relates the concerns of a typical educated man of the Sung period about a wide

range of interpersonal, educational, social, and economic matters, including moral self-cultivation, clothing styles and manners, basic legal knowledge, bringing up children and arranging their marriages, managing family property and financial dealings, relations and feelings among family members, the handling of slaves and tenants, and the like. As a whole, the *Shih-fan* reveals some of the basic elements of the everyday thinking of the upper class in imperial China. Ebrey's translation has thus made this valuable source material available to both general scholars and China experts who cannot read or have difficulty in reading literary Chinese. Second, from the standpoint of recent scholarship on Sung China in this country, Ebrey's book is a useful complement to the recent studies of the life, social world, and ideas of the Sung elite by Robert P. Hymes, Richard L. Davis, John W. Chaffee, and the present reviewer. While these studies have focused on the Sung elite in their larger political, social, and cultural world, Ebrey's work is on their frame of mind in dealing with routine matters of everyday life in their private and intimate family world.

Finally, I must evaluate the translation and the scholarship that Ebrey has devoted to the annotations and the book as a whole. In general I find Ebrey a faithful and reliable translator and her book a good scholarly work. But I have also found a number of serious mistakes in her translation of idiomatic or complex expressions, in her understanding of special terms, and in her notes on scholarly issues. Four such errors are given here as examples. On page 179, in translating Yüan Ts'ai's short note on Cheng Ching-yüan's comments on the choice of a proper title for the book, the original Chinese *kan-pu* and common idiomatic expression *kan-pu ching-ts'ung* are misunderstood, and the line is incorrectly rendered as "Although I did not dare follow it, I inscribe it here." It should be "Could I dare not to respectfully obey [or follow] it? Hence I now inscribe . . ." Because of this misunderstanding, Ebrey's reading, and translation, of the line that follows is erroneous and misses the real intention of Yüan Ts'ai's statement. On page 177, the translation of *shih-hua* as "anecdotes about poets" is incorrect. The term *shih-hua* carried two different meanings as a literary genre in traditional times. One was a form of literary criticism that consisted of a critic's comments on various aspects of Chinese poetry, which could be rendered as "talks on poetry" or even "anecdotes about poets," as is given by Ebrey, since it often included anecdotes about the poets under consideration; the other meaning of *shih-hua* refers to the popular tale narrated in prose and punctuated with verse that became popular in

Sung times and has been commonly translated as "popular tale with poems." Yüan Ts'ai clearly used *shih-hua* in its second meaning, for it went together with *hsiao-shuo* (stories), and he considered it to be "of no use in moral instruction," a traditional Chinese view on fictional work and similar writings.

Commenting on Yüan Ts'ai's reference to the *Chung-yung* (*The Doctrine of the Mean or Centrality and Commonality*) in note 3 on page 178, Ebrey states that the *Chung-yung* is "one of the thirteen classics as established by Sung Neo-Confucians." This is incorrect. The *Chung-yung* was not one of the thirteen classics, although it was part of one of the thirteen classics, the *Li-chi* (*The Book of Rites*). In fact only two of the *Four Books* were counted as independent units of the thirteen classics.

For an old text such as Yüan Ts'ai's *Shih-fan*, its editions and their transmission since the twelfth century are of critical importance to a scholar who studies and translates it. Ebrey has shown her research on this difficult problem in her long essay "Editions of the *Precepts for Social Life* and Their Transmission" at the end of her book. Her coverage of materials is comprehensive. But mistakes also exist. For instance, in discussing the *Ts'ung-shu chi-ch'eng* edition of the *Shih-fan* (1939) on page 328, she states that it is based on the *Chih-pu-tsu chai ts'ung-shu* edition and "includes Ming and Ch'ing colophons but not Yang Fu-chi's preface." Yang Fu-chi's preface (dated 1788) is in fact included in this edition; it is printed on the second page of the volume and has seven lines.

CHUN-SHU CHANG
University of Michigan

GAO YUAN. *Born Red: A Chronicle of the Cultural Revolution*. Stanford: Stanford University Press. 1987. Pp. xxxii, 380. Cloth \$39.50, paper \$7.95.

NIEN CHENG. *Life and Death in Shanghai*. New York: Grove. 1986. Pp. ix, 547. \$19.95.

Both authors have written memoirs of their experiences during China's Cultural Revolution (1966–1976), but there the similarity ends. Gao Yuan, a former Red Guard, concentrates on his experience as a middle school student during the most violent phase of the movement (1966–1969). Nien Cheng, a retired employee of the Shanghai branch of Shell Petroleum, details her arrest, imprisonment, release, and eventual departure from China in 1980. The profound differences between these two books reflect the generation, class, gender, and life experience of the two authors; the differences are also a product of the narrative strategies each adopts in reconstructing the events of the

Cultural Revolution in English for a Western audience.

Gao (he preserves the Chinese word order wherein surname comes first) was fourteen years old when the Cultural Revolution began. The son of a dedicated rural administrator and an accountant, he was a boarding school student in a small town in northern China. Without sentimentality or overt nostalgia, he re-creates the town of his youth and the all-absorbing world of school, in which adolescent jokes about sex loomed almost as large as political education. For Gao, the Cultural Revolution started inconspicuously enough as a campaign to criticize certain articles in the press, a movement endorsed by the Communist Party and led by the teachers. But the campaign spread quickly to criticism of the same teachers for errors ranging from intellectual eclecticism to sexual misconduct. In a world where the authority of teachers and parents alike had been repudiated, the heavily armed students turned finally to fratricidal warfare.

Although many memoirs of the Cultural Revolution have been published in recent years, *Born Red* stands out for the immediacy of its portrait of the Red Guards. Without ever abandoning the voice of someone on the edge of childhood, Gao Yuan creates a nuanced and complex picture of the lives of his peers. As individuals and factions, they were capable of extreme dedication, courage, generosity, selfishness, and murderous violence. Gao does not provide a unitary explanation of why the Cultural Revolution became, for a brief period, a frenzied campaign of adolescent warriors. But *Born Red* offers evidence of many factors contributing to their passion: a particular type of political education, incipient class tensions among the students themselves, political manipulation by the Chinese leadership (itself badly divided), and the enticements of an environment devoid of adult regulation. Ultimately, Gao's narrative is powerful, compelling, and deeply disturbing precisely because it refuses to impose retrospective interpretation or an adult voice on the events of those years.

In 1966, Cheng (unlike Gao, she adopts the Western custom of placing family name last) was a middle-aged woman living in comfortable, even opulent, retirement in Shanghai. Because she and her deceased husband had formerly worked for Shell—and, one suspects, because of her obvious wealth—her home was ransacked by Red Guards. Soon thereafter she became a victim of state violence as well, when she was arrested and detained for six-and-a-half years. Released as abruptly as she had been imprisoned, she spent the remainder of the 1970s seeking the truth about her daughter's alleged suicide (it was later ruled a murder).

The last section of the book records her adjustment to the crowding and favor-trading of post-Cultural Revolution Shanghai, and her detached amusement at the rise in her social status as China moved into the post-Mao period.

Like Gao, Cheng is a skilled narrator who draws the reader into her tale. Although the unself-conscious and unrepentant elitism of her opening chapters is jarring, by the time she is imprisoned she has captured the reader's sympathy and outrage. Her re-creation of the routine cruelties of political imprisonment recalls Arthur Koestler's *Darkness at Noon*, and her stubbornness, courage, and persistence in the face of physical and mental abuse compel the reader's admiration.

But the urge to identify with Nien Cheng or measure one's own character against hers is also disturbing, for bundled with her story of individual courage is an explanation of the Cultural Revolution that is far from complete. Whereas Gao eschews analysis and forces the reader to wallow in the disorder, chaos, and senselessness of much of the Cultural Revolution, Cheng offers an analysis that American readers will find comforting because it is so familiar: this is the inevitable behavior of Communist tyranny. Gao reconstructs with very little comment both the class tensions and the psychological flaws that made his classmates turn on one another. Cheng, on the other hand, privileges one explanation for the conflict: the premeditated and highly organized resolution of a factional conflict among top Communist leaders that reached down to disrupt the lives of ordinary citizens like herself.

Any comprehensive historical account, of course, ultimately will have to deal with all of these aspects of the Cultural Revolution and others as well. But as a memoir of one person's particular experience that hints at the disturbing complexity of the whole, Gao's book is ultimately more successful. Gao baffles and disturbs by making us examine the Red Guard in ourselves; Cheng simplifies and comforts by assuring us that such behavior could only come from a Communist other. Those who find this explanation congenial will not have to look further for the origins of the Cultural Revolution. Those who are dissatisfied with Cheng's formula can still applaud the courage, articulateness, and skill of both authors.

GAIL HERSHATTER
Williams College

YOSHIOHARU Tsuboi. *L'empire Vietnamien face à la France et à la Chine, 1847-1885*. Foreword by GEORGES CONDOMINAS. (Recherches Asiatiques.) Paris: L'Harmattan. 1987. Pp. 291.

In order to appreciate this useful book—a study of nineteenth-century Vietnam written in French by a Japanese scholar using Vietnamese, Chinese, Japanese, French, and English sources—it is not necessary to agree with the fatuous supposition noted on the back cover that Yoshiharu Tsuboi has provided us with an "Asiatic perception" of his topic, for of course he has done no such thing. His perception was as much shaped in "the West" as in "the East" and is an admirable but by no means unique illustration of how thoroughly internationalized historical scholarship on Southeast Asia has become.

The chief purpose of this treatment of the Tu Duc reign (1847-83) is to correct the prevailing negative and still largely colonial-minded view of the period and even of the ruler himself, who in Western works has appeared as "half-mad," feckless, and incompetent. (It is worth noting that Tu Duc fares little better in the works of many Vietnamese historians.) Tsuboi accomplishes his task in stages, treating first relations with the French, then the Chinese, and then the internal relations of the Vietnamese empire and its society. This scheme of organization produces several difficulties, among them an annoying repetitiveness and a chronological muddle, and of course does not diminish the very compartmentalized thinking that Tsuboi implies he wishes to avoid. Nevertheless, the pages concerning the French provide a succinct and, in a few instances, corrective version of the conventional colonial history, enhanced by several well-chosen, illustrative quotations newly mined from the colonial archives. The section on relations with the Chinese offers a much better basic explanation of the variety of their involvements in Vietnam, and a clearer account of the Black, Yellow, and White flags, than is easily available anywhere else. The Vietnamese chapters, comprising a little more than half of the book, present a spirited and sympathetic portrait of the Nguen court and the society around it. Especially helpful are the short biographies of prominent supporters and detractors of the emperor, the discussion of the court's political intimacies, and detailed information on the nature and activities of the educated elite. Some of the material and perspectives are new. The problem of Tu Duc's ascension to the throne, for example, is viewed in a fresh light. Other features in the text, while by no means new, use data obscured by time or hidden in archives to make particular arguments with new force. For example, details on the examination system, not easily available for this period, are here assembled from printed and archival sources in order to support the view that the literati's role in determining the character of the period was even greater than generally allowed.

There is neither index nor glossary, and the bibliography, for no apparent reason, is divided by language. But these sins of omission and commission are redeemed by the model effort to use Sino-Vietnamese and Japanese characters, accompanied by romanization and alternate Vietnamese and Chinese readings, for all important names, terms, and bibliographic items.

WILLIAM H. FREDERICK
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ANITAINDER SINGH. *The Origins of the Partition of India, 1936–1947*. (Oxford University South Asian Studies Series.) New York: Oxford University Press. 1987. Pp. xiv, 271. \$21.95.

This is an able study based on the author's Oxford D.Phil. thesis. Her task is to investigate the partition of the Indian subcontinent into Muslim Pakistan and secular (but predominantly Hindu) India on the departure of the British in 1947—a political decision that displaced millions and was accompanied by horrific communal carnage. Strong, often ideologically motivated, arguments have been used to explain Partition: British machinations to divide India's communities in order to rule or the "inevitability" of partition on the grounds that Hindus and Muslims constituted separate nations. Anita Inder Singh convincingly shows that Indian Muslims were as heterogeneous as the Hindus of the subcontinent, divided by social and economic standing, by theology and by region; and that Partition was not historically inevitable. Certainly, there was not a united Muslim political community with a common national aim, even as late as 1945. The main strength of the book is its meticulous account of the interweaving of regional and all-India Muslim politics and the way in which the All-India Muslim League, led by M. A. Jinnah, progressed from being an electoral flop in 1936–1937 and certainly not the mouthpiece of all Muslims to being the recognized voice of Muslims in the negotiations leading to the transfer of power. (In this connection, the book should be read with A. Jalal's study of Jinnah, *The Sole Spokesman* [1985].) Singh struggles to explain why the Muslim majority provinces were at last prepared to swing behind Jinnah although local political strategies had made good sense even as late as 1946. She portrays a divided and disorganized Muslim community whose leaders and rank and file had widely differing and often hazy ideas about what "Pakistan" might mean; a Congress party that profoundly underestimated Muslim feeling and fear, whose leaders were no negotiating match for the wily Jinnah, a past master of stalling and misleading his opponents in negotia-

tion; and a weakened British raj whose political masters in Whitehall tried until the end to maintain a united India that could still through treaty arrangements buttress a British worldwide presence and power. It was the interplay of these three forces in the specific circumstances of world war and the effect of the conflict and its aftermath on the British that precipitated the state of Pakistan, which few wanted or thought was viable except in some loose linkage with India. The 1942 Cripps Mission—a wartime expedient to bolster British power and international standing—opened the door at the all-India level to the destructive leverage Jinnah was able to exert. Singh suggests that at a popular level the communal massacres of late 1946 were the point of no return: blood shed, homes destroyed, suspicions confirmed in that slaughter could only be redeemed and resolved by a partition on religious lines, whatever the human price still to be paid.

The great problem with this study stems from the papers of Jinnah and the league not being available. An understanding of Jinnah's intentions and the compulsions of regional politics that ultimately gained him local Muslim support—but only on all-India issues—therefore has to be built up from the documents left by the Congress Party and the raj and from the copious newspaper coverage. Singh's use of newspaper material is particularly valuable. She has comparatively little on the dynamics of Congress party politics—understandably, as there are available accounts of this aspect of events. But she leaves many important questions unanswered about the linkages between central and provincial Muslim politics and in particular the way in and degree to which Pakistan became a truly popular cause in 1946–1947. To take one example, where did the central league obtain the money it poured into fighting the elections in Punjab in 1946? Indeed, was Pakistan ever a mass political demand rather than a rallying cry of the terrified? Had Jinnah in fact let loose a tiger he could not control by using an undefined Pakistan as a negotiating chip? What manner of man was Jinnah, and what made him the aloof, much disliked negotiating wizard who conjured Pakistan out of a disparate group of co-religionists in the face of powerful Muslim suspicion, hostility from the Congress party with its all-India majority, and the priorities of the departing rulers? Despite the unanswered questions, students will be grateful to the author for the clarity of her work and for gathering together previously scattered arguments and evidence, as well as for demonstrating the remaining lacunae in the historian's understanding of Partition.

JUDITH M. BROWN
University of Manchester

JOHN MERRITT. *The Making of the AWU*. New York: Oxford University Press. 1986. Pp. x. 432. \$38.00.

This book deals with the early years of the struggle to develop trade unionism in the Australian pastoral industry. The Amalgamated Shearers' Union of Australasia (ASU) was created in 1887 and became the Australian Workers' Union (AWU) in 1894. "The AWU's struggle to establish organisational stability is . . . [John Merritt's] major theme" (p. 2).

By 1920, the AWU had become a large general union, and over the next four decades it dominated the growth of unionism in Australia. Merritt concentrates on the pastoral industry phase during the years to 1911 while the AWU comprised largely shearers and shedhands.

Chapter 1 traces the fortunes of the pastoral industry during the period 1860–1911. Chapters 2 and 3 provide useful sketches of "shearing and shearers" and "the shed floor" in the 1880s. Against this background, the remaining chapters, 4 to 11, describe developments generally in chronological order. Chapter 4 concerns the establishment of the ASU to provide organized opposition to the pastoralists' proposed cut in shearing rates. Pay, working conditions, membership, and the degree of AWU control remained the central issues. Merritt often quotes figures on pay rates and membership. Tables listing these data for the period would have helped.

Chapter 5, titled "Port Blockades and the Closed Shop," and covering the years 1887 to 1890, describes sympathetically the union's failure to prevent non-union shearing. This lack of success contributed to the maritime strike (involving seamen, wharf-laborers, and coal lumpers) from August to November 1890. The ASU encouraged wharf laborers to refuse "to load wool from non-union stations" (p. 154). Merritt relates well how the strike more strongly united the employers. Shearers themselves were on strike for only a week. Their position was weakened by the plentiful supply of non-union labor. At the end of the strike, it was recognized that what pastoralists had been fighting to retain, namely, "'Freedom of contract' [the title of Chapter 6] had triumphed" (p. 164).

Merritt could have brought out more clearly how the severe depression in Australia during 1891–1893 became a more important influence than the failure of the strike in the temporary decline of unionism in Australia. The strike and depression and then the great drought of 1895–1903 set the scene for the AWU's struggles described in the next three chapters. A major new problem in 1902 was the establishment of a rival union that Merritt believes, largely on grounds of

"circumstantial evidence" (p. 299), was "a 'bogus' union" (p. 299).

Out of the challenges of the 1890s and the defeat of the unions industrially came two institutional developments that have greatly shaped Australia's political and industrial history. First, the unions sought—with increasing success from 1891 on—to achieve their objectives with the foundation of the Parliamentary Labor Party. Second, the industrial struggles influenced public opinion and legislation introducing compulsory arbitration. These developments form the subject of the penultimate chapter and contribute to the final chapter, "Organisational Stability."

In relating his findings, based largely on union and pastoralists' records, Merritt mentions, it seems, the names and roles of all the participants from both unions and pastoralists. This contributes to dull reading at times, since there are so many that it is often difficult to remember their roles, and we are not always immediately reminded of them.

The chronological treatment makes for a disjointed and fragmented treatment of major developments, including of the two institutions noted above and of the rise and fall of the rival union. More allowance could have been made for Australia being a big country with a sparse population. Nevertheless, Merritt's study adds considerably to the history of the AWU's achievement of organizational stability.

ERNST A. BOEHM
University of Melbourne

UNITED STATES

JOHN D'EMILIO and ESTELLE B. FREEDMAN. *Intimate Matters: A History of Sexuality in America*. New York: Harper and Row. 1988. Pp. xx. 428. \$24.95.

A work of extraordinary scope and boldness, this book offers a sophisticated and sweeping interpretation of how the meaning and place of sexuality has changed in American life from the colonial period to the present. Drawing on court records, vice investigations, medical texts, personal memoirs, survey research, and prodigious reading in the recent scholarship on the history of sexuality, the authors have written a lucid synthetic narrative that deserves to find a wide audience among scholars and students interested in women's history, sexuality, the family, and popular culture.

The book covers a staggering number of topics. These include such topics as seventeenth-century

English beliefs about sexuality, European settlers' responses to Indian sexual customs, prostitution, venereal disease, courtship, the interplay of racial and sexual ideology, sex and marriage in utopian communities, same-sex intimacy, the growth of commercialized amusements, the rise of homosexual subcultures, and liberalization of censorship laws.

The book's overarching argument is that, over the past three centuries, the place of sexuality has undergone far-reaching transformations. In the colonial period, marriage was regarded as the proper place for sexuality and procreation regarded as sex's proper function. By the mid-nineteenth century, as a growing acceptance of contraception loosened the link between sex and reproduction, the discourse on sex increasingly associated sexuality with mutuality, personal intimacy, and spiritual transcendence. Since the late nineteenth century, the tie between sexuality and marriage weakened further as the growth of cities undermined small-town behavioral controls and sexual norms, as young people gained increasing autonomy, as the dislocations of war increased opportunities for sexual relations outside of marriage, and as a growing consumer culture encouraged "an acceptance of pleasure, self-gratification, and personal satisfaction" (p. 234).

As sexuality's place in Americans' lives changed, so too did systems of sexual regulation. During the seventeenth century, the church, courts, and community all shared a responsibility to monitor sexual behavior and punish illicit behavior. By the mid-eighteenth century, familial and community regulation of sexuality had declined, manifest in rising rates of illegitimacy and premarital pregnancy, the abolition of many church courts, and declining legal prosecution of sexual offenses. Gradually, the medical profession and voluntary associations, such as the nineteenth-century moral reform and social purity movements, came to play a larger role in regulating personal morality, a role supplemented beginning in the late nineteenth century by the state and by popular culture.

No one will close the book without a deep respect for the authors' sociological sophistication, their breadth of interests, and their mastery of the historical literature on sex. Yet precisely because the authors are willing to spell out their argument so clearly, their interpretation is bound to stimulate debate. Their explanation of the causes of change is sure to provoke controversy. For John D'Emilio and Estelle B. Freedman, the most powerful engines of change are broad-based social and economic forces. Yet this argument will lead readers to ask how slow, gradual socioeconomic developments can explain abrupt fluctuations in sexual behavior. Their contention that the underlying

causes of change were social and economic rather than intellectual leads them to sharply downplay the role in the twentieth century of sex researchers, marriage counselors, and birth control advocates in challenging prevailing notions of morality, breaking down the nineteenth-century "medical model" of sexuality, and creating a new sexual ethic—the individuals who were the critical actors in earlier accounts of sexual reform such as Sidney Ditzion's *Marriage, Morals, and Sex in America* (1953) and James Reed's *Birth Control Movement and American Society* (1978). *Intimate Matters* is very much a work of social history, not intellectual history, and it devotes little attention to the ideas and impact of Sigmund Freud, Robert Latou Dickinson, Emily Mudd, Margaret Mead, or Ruth Benedict.

The book's focus on group behavior and social causation also leads the authors to touch only lightly on issues of individual psychology. What, one might ask, was the meaning and place of sexuality in specific individuals' lives? To take just one example, Nathan Hale has shown that the late nineteenth-century crisis of civilized morality was at once broadly social and intensely personal. Many individuals—including the leading sexual reformers—were riddled with guilt and sexual anxiety. This deeply conflicted psychological atmosphere created a climate in which a new modernist sexual ethic took root.

The authors' explanatory scheme places much of the emphasis on working-class youth and on free lovers and anarchist sex radicals as sources of new standards of sexuality. It is difficult not to feel that they overstate the influence of these groups and the extent to which the working class rejected the "middle class" ethic of female premarital chastity. They only mention a single ethnic group, Italians, that used familial and community pressure to maintain low rates of illegitimacy.

The book, for all its breadth, does have one major omission: the institutional response to the late nineteenth-century crisis of civilized morality. After the turn of the century, major foundations, including the Milbank Memorial Fund, the Scripps Foundation, and the Rockefeller Foundation, began to fund research on contraception, endocrinology, human sexual physiology, and population problems, as well as sex education and marriage counseling. The authors say almost nothing about the National Research Council's Committee for Research on Problems of Sex, the Bureau of Sex Hygiene, or the Committee on Maternal Health—the institutions that succeeded in overcoming the opposition of the medical profession toward contraception, revising legal statutes, improving birth control methods, and opening birth control clinics. By largely ignoring the

role of such institutions, they fail to consider adequately one of the major forces for change as well as the competing agendas behind sexual reform: female autonomy, better sexual adjustment in marriage, acceptance of sexual diversity, and a desire to reduce birth rates among the poor.

These observations in no way detract from the book's importance or originality. By drawing together the latest historical research and placing this scholarship in a sophisticated conceptual framework, D'Emilio and Freedman have placed historians in their debt and have made a significant contribution to our understanding of Americans' private lives.

STEVEN MINTZ
University of Houston

BARBARA MEIL HOBSON. *Uneasy Virtue: The Politics of Prostitution and the American Reform Tradition*. New York: Basic Books. 1987. Pp. x, 275. \$20.95.

Barbara Meil Hobson has written a highly readable but carefully documented book on politics and reform in the world of nineteenth- and twentieth-century American prostitution. Although she gives a clear picture of changing attitudes and laws throughout American history, Hobson highlights three periods: the religious revival of the 1840s; Progressive reforms, 1900–20; and the feminist era of the 1970s. Enhancing her arguments are comparisons of laws, policies, and reforms in European countries.

Hobson discloses the association between the economic health of cities such as Boston and the world of prostitution. Where prostitution is seen as a threat—for example, where the prevalence of brothels lowers property values—there are demands for reform. Because the profits of prostitution go far beyond the individual prostitute, however, demands for reform merely cause a shift in the institution, not fundamental changes. For example, the outcries against vice and especially prostitution by the religious reformers of the 1830s and 1840s led the police to concentrate more and more on the female prostitute, the most visible element. In earlier times both men and women had been stigmatized by vice crimes, but the view evolved that “female promiscuity was linked to a criminal enterprise” (p. 35). In fact, by 1866 the Maine Supreme Court went so far as to define a prostitute as “a female” and prostitution as “the common lewdness of a female” (p. 35).

Attitudes, of course, were not changed solely by reformers influencing the courts. Fiction played an important role throughout the nineteenth century in establishing an image of the prostitute as a victim of poverty or more often of seduction by an

unprincipled man. Hobson offers us fascinating case studies that show how even the courts were swayed by the fictional image.

During the Progressive era there was an “intensive crusade” against prostitution, making it both a public and a private concern. Like most Progressive reforms, however, the crusade against prostitution was confused by the multitude of views on causes and solutions. Hobson does an excellent job of explaining the confusion among reformers.

Confusion can be found even among the feminists of the mid-twentieth century. On the one hand, many saw prostitution as the ultimate exploitation of women and felt compelled to work for eradication. On the other hand, some feminists, recognizing that vice laws were enforced only against women, not only protested the double standard but also argued in favor of women controlling their own bodies, lives, and sexuality. The fact that prostitutes started speaking up for themselves by the 1970s merely added a new dimension to the issue.

The contradictions of prostitution laws and policies have not lessened as a result of new ways of looking at and defining women's roles and sexuality. According to Hobson, “Prostitution policy remains at an impasse” (p. 234).

Hobson has written the best kind of social history—a nice mix of lively narrative, factual and statistical information, and amusing and poignant anecdotes.

JACQUELINE BAKER BARNHART
California State University,
Chico

STEVEN MINTZ and SUSAN KELLOGG. *Domestic Revolutions: A Social History of American Family Life*. New York: Free Press. 1988. Pp. xx, 316. \$22.50.

Steven Mintz and Susan Kellogg here offer an appealing and erudite summary of American family life from the colonial period to the present. For the last few decades, the renaissance of family history has produced mainly monographs, and there is now plenty of material on which to base a synthesis. This is a state-of-the-art review of U.S. family history, demonstrating a prodigious mastery of the scholarship. The limitations of my own knowledge make my judgments tentative, but in several areas I could see that the authors have used the best and most recent monographs, on issues ranging from childbirth to boarding to divorce. The book is enriched and enlivened by sensitivity to the wide range of experiences that affect family life, never projecting an image of families as insulated. It includes material on work-

ing conditions, women's participation in employment, mechanization of agriculture, conditions of slavery, for example.

The introduction argues that the United States has always had a diversity of family forms, and that the historical shift most shared among these forms over the last three centuries has been from families that were the basic economic and political units of the nation to families that function primarily to provide nurturance for individuals. Unfortunately, neither argument is thoroughly developed, and the book remains more descriptive than analytic.

The authors rely on the concept "diversity," which has recently become a code word for racial, religious, and ethnic difference and discrimination. The book absorbs some of the defects of this concept and falls into a pluralist paradigm, implying freedom of preference among diverse families and masking the fact that many differences are actually inequalities. Moreover, even the benign aspects of diversity are not well treated in this book because its structure supports the notion of a normative family and exceptions to it. There is, for example, an excellent chapter on the Afro-American family, but it is presented as a nonstandard family form. Throughout the book, material on non-"WASP," non-prosperous families appears not in the main flow of the narrative but in separate paragraphs, which read as digressions. For example, discussions of child rearing do not mention the substantial scholarship on class differences in parenting. In the chapter on industrialization and the working-class family, there are no blacks. There is very little on homosexuality; lesbians are not in the index. Single motherhood appears for the first time in the 1960s and then only as a subcategory of poverty. Chapter 1 is about the Puritans; in Chapter 2, "The Roots of Diversity," we meet Native Americans, Afro-Americans, and southern whites.

Another weakness of the book stems from its very strength: because so much is included, much of the core of what we think of as familial is underdeveloped. There is no discussion of family violence, little of changing marital and sexual relations. "Family" is used grammatically as if it were a unit; gender and generational relations and conflicts within families are not prominent. The nuclear form of family is an unproblematic given, and there is little discussion of relations with extended family. (Oddly, "extended family" is defined in a brief glossary/appendix, but I could not find it mentioned in the text.) This was a surprise, given that Kellogg is an anthropologist.

Because of the failure to develop the arguments set out in the introduction, and the desire to include so much, the book becomes homogeneous

in texture and lacks peaks and valleys, like a textbook, albeit a very good one. Contributing especially to this blandness is the relative absence of conflict. Change happens inexorably, if not steadily. For example, the family wage system, in which men are expected singlehandedly to provide for wives and children, is well defined as a new development. But since its causes, inconsistencies, and consequences for men, women, and children, and for different classes and races, are not delineated, the implications of this transformation are lost, and the process appears consensual. There is little delineation of the agency of change; the study is weak in its handling of the social movements and cultural conflicts that contributed to family change. There are too many references to "Americans" as unified subjects, as in "Americans had simply rejected a series of older family ideals" (p. 114, about the period 1900–1930). Welfare legislation in the New Deal, for example, is discussed as it mitigated poverty, but its impact in restructuring families is not examined.

These criticisms are not mere quibbles but neither do they detract from the value of this book as a learned synthesis; I expect to use this study frequently. It is a rich mine of information and bibliography, extremely well written and organized. Its descriptions are never simplistic. I hope the authors go on to produce more interpretive work on the family, something for which they are extremely well equipped.

LINDA GORDON

University of Wisconsin

SAM D. GILL. *Mother Earth: An American Story*. Chicago: University of Chicago Press. 1987. Pp. x, 196. \$24.95.

In this carefully focused exercise in ethnographic, ethnohistorical, historical, and textual sleuthing, Sam D. Gill, following the model of Olof Pettersson's *Mother Earth: An Analysis of the Mother Earth Concepts according to Albrecht Dieterich* (1967), argues that the notion of a female figure "Mother Earth" commonly attributed to North American Indian religions was the construct of European Americans beginning around 1820. The ethnographic sources, which he has thoroughly surveyed, yield many goddesses and figures of feminine identity, from the Hopi Spiderwoman to the Pawnee Mother Corn, but, Gill argues, these should not be collapsed into a single, primordial, or archetypal theological concept.

Gill's investigation reduces the actual sources for such a concept to a mere five, all of them of highly questionable authenticity, which he ex-

plores in methodologically instructive chapters. The most critical and suggestive chapter, however, is chapter 6, titled "The Making of Mother Earth: The Scholars." Beginning with the work of anthropologist Edward B. Tylor and working his way thoughtfully through the ideas of George Bird Grinnell, James Frazer, Alfred L. Kroeber, Mircea Eliade, and Åke Hultkrantz, Gill reaches a devastating conclusion: "It would not be inaccurate to criticize or even to dismiss a century of scholarship on Mother Earth as inadequate, even irresponsible, and certainly inconclusive" (p. 121).

Mother Earth, he concludes, has been a product of Euro-American rather than Native American minds, "part of a creative enterprise reflecting the needs of the storytellers" (p. 122) and closely associated with the political and psychological process of settlement and expropriation of land in American nationalist history. American mythmakers, from journalists to ethnographers to professors of comparative religion, perpetuated only too easily an evolutionary and hierarchical vision of society and history: because "primitive" peoples anthropomorphize the physical world in a childlike manner, there must have been such a widely shared theological figure as Mother Earth; and, because civilized people rationally pursue generalization in preference to the local and immediate, it is our concern to find not the particular but the universal. Following this double compulsion, scholars over the past century gradually established the story of the Indians' Mother Earth.

Gill concludes with a review of the various ways in which Native Americans, especially since 1950, have themselves built on the white-created myth and adopted it for their own purposes of politics and identity. "All of these lines show," he concludes, "that Mother Earth has come to be a major goddess to the Indians within this century and that her development has been necessarily dependent not only upon the crises caused by Americans of European ancestry but also upon European American interpretations and expectations of Indians and their religions" (p. 150).

If there is a major shortcoming in Gill's treatment, it is a lack of attention to issues of gender and power. The conflation of Indian with Mother (female) and Earth (land) and their association, as in the Tecumseh story, with positions of seating on the ground or subordinate placement, in contrast to the white-male-father-sky-sun associational complex and its positioning on chairs (civilization)—such sets of contrasting configurations—suggest that in the storytelling of the nineteenth century not only the Indians but also the Earth itself and all things-persons feminine were being put in their places. Gill points the way here, in

short, to a rich symbolic world of power, dominance, and subordination.

CURTIS M. HINSLEY
Northern Arizona University

RUSSELL THORNTON. *American Indian Holocaust and Survival: A Population History since 1492*. (Civilization of the American Indian Series, number 186.) Norman: University of Oklahoma Press. 1987. Pp. xx, 292. \$29.95.

We have needed this book for a generation. The accumulation of publications of scholarly worth on the subject of Amerindian demography has long been overwhelming for the general reader and daunting for even the specialist. Even if the subject is restricted to the population history of indigenes north of the Rio Grande, the focus of roughly 90 percent of Russell Thornton's new book, the pertinent bibliography is huge. Now, at last, we have a survey to recommend to the novice and to consult ourselves when we lose our way. After a few score pages on the arrival of humans in the Americas and the probable size of the Amerindian population in 1492, the book turns to its main subject: the depopulation, disease, alcoholism, and genocide of the native peoples of the Americas. The subject is depressing in the extreme, but we must consider it if we are ever to rid ourselves of the distortions of the orthodox and selective version of our history. The book's final chapters deal with the demographic recovery of the Amerindians in the last century, their increasing urbanization, and such interesting matters as declining tribal loyalties, increasing intermarriage between Amerindians and non-Amerindians, and the probable demographic future of Native Americans. Interspersed among the discussions of demography and censuses are illustrative minihistories of such main events of Amerindian history as the smallpox epidemic of the 1830s and the two Ghost Dance movements, along with brief sketches of the historical trials of various tribes, from the swarming Navaho to the Kickapoo, that have always managed to avoid extinction—but barely.

One of the most valuable aspects of this book is its numerous specific references to its thirty-four-page bibliography. If the reader disagrees with the author's statements, he or she can easily locate their sources. The bibliography is, of course, not complete. For instance, as I leaf through it, I find nothing in Spanish, although the sixteenth-century sources on the subject are often in that language, as are many twentieth-century works. And, if one mentions African immigration to America, however briefly, where are the refer-

ences to Philip Curtin's research? And, even if one writes no more than a few paragraphs on the Huron, how could one fail to cite Bruce G. Trigger's *The Children of Aataentsic* (1976)? But I will be consulting this bibliography often in the coming years, and I am an ingrate to carp about its few omissions.

Any book of this scope is bound to be open to attack on specific points because it can be no better than the sources, usually secondary, on which it is based. The epidemics of cholera cited as occurring in America before the 1830s cannot have been of the disease, Asiatic cholera, to which that word almost invariably refers today. It did not cross the Atlantic until the 1830s. The word "cholera" was long used in reference to a kind of summer dysentery, which can be fatal for the young, but which is never as generally lethal as the Asiatic cholera that burst out of the Ganges valley and wreaked so much damage all over the world in the nineteenth century. There is no greater curse for medical historians than the name that refers to more than one disease or the disease that has more than one name.

But I am straining to live up to my role as a critical reviewer. This book belongs on the shelf of every Indianist and buff of Amerindian history. It is one of the essential reference books.

ALFRED W. CROSBY
University of Texas

JOHN L. KESSELL. *Kiva, Cross, and Crown: The Pecos Indians and New Mexico, 1540–1840*. Albuquerque: University of New Mexico Press. 1987. Pp. xiii, 587. \$17.95.

A quotation from a paragraph by the author reproduced from the first preface (1977) may serve as a short guide to the present book: "Here . . . is a beginning, a historical documentary of the eastern fortress-pueblo from earliest Spanish contact in 1540, to abandonment three hundred years later." The book had to be fast-paced to cover three hundred years of Spanish and Native American history in New Mexico and provide the essential connections with Mexico. Americans think a nod to 1540 and Governor Francisco Vázquez de Coronado is an adequate early start, but John L. Kessell included Nuñez Cabeza de Vaca and his black companion, Estebanico (1528–1536). Estebanico guided Coronado to Zuni but then gave his life for the service (p. 7).

Coronado learned the most about Pecos-Cicuye and left a priest, an old Franciscan lay brother, two Tarascan servants, and a Portuguese soldier in Pecos when he returned to Mexico in 1542. Governor Antonio de Espejo reestablished contact

with New Mexico in 1582–1583. A real Spanish colony was moved from Mexico to New Mexico by Governor Gaspar Castaño de Sosa, arriving in Pecos in December 1590 (p. 48). Soldiers sent by Viceroy Velasco to New Mexico to arrest Castaño de Sosa arrived and took him back to Mexico City. He was then sent to the Philippines.

Governor Juan de Oñate led the next exploring and missionizing expedition into New Mexico on April 30, 1598. Passing Pecos pueblo, Oñate established his headquarters on the Rio Grande about fifty miles northwest of Pecos, where he dedicated the first church in New Mexico on September 7, 1598. After a dozen Spaniards were ambushed and killed at Acoma, Oñate ordered that all Acoma males twenty-five and older would have one foot cut off. Like other young men and women, these defeated and mutilated warriors were in addition sentenced to serve twenty years as slaves of the invaders (pp. 84–86). Following a large expedition to the Great Plains, most of the Spanish colonists left New Mexico and returned to Mexico (pp. 89–90). On August 24, 1607, Don Juan de Oñate resigned his governorship but had to wait until February 1610 to receive his successor, Don Pedro de Peralta (1610–1614).

The fact that only four governors ruled New Mexico in the first seventy-seven years of Spanish control gives a very false idea of the rapid change of the highest Spanish officials in New Mexico that followed. Starting in 1617, the power of the Franciscan missionaries became strong enough to challenge that of the civilian governor and the soldiers he had under his command. In January 1626, Fray Alonso de Benavides was named "custos," or supervisor, of the Franciscans and agent of the Inquisition, thus doubling the power of the church vis-à-vis the civil authority.

Most of *Kiva, Cross and Crown* is devoted to the struggles among the Spanish governors, inquisitors, and Franciscan priests. The job of supervising non-religious activities of the Indians was given little attention, yet the relations of various Apache and Comanche visitors from the Plains were well documented, and some mention of Navajo, Ute, and Wichita visits to the Pueblo Indians were recorded.

One of the most interesting and informative sections of the book is Chapter 6, dealing with the Pueblo Revolt of 1680, which Kessell blames on Governor Otermin. The success of Governor Don Diego de Vargas Zapata Luján Ponce de León y Contreras from February 1691 to July 1697 regained the loyalty of the Pueblo Indians and reestablished thriving Spanish communities. Diego de Vargas was replaced in 1697 because of poor communication between Santa Fe and Mexico City. Diego de Vargas was governor of New

Mexico for the second time. March and April 1704, but he died during a campaign against the Apache (p. 296).

The one serious omission is an appendix with a chronological listing of the Spanish governors with dates of service.

OMER C. STEWART
University of Colorado

DAVID B. GRACY II. *Moses Austin: His Life*. Foreword by MARY AUSTIN PERRY BERETTA. San Antonio, Tex.: Trinity University Press. 1987. Pp. xx, 303. \$24.95.

This biography of Moses Austin is one of the most significant books on the early history of Texas to appear in decades. Austin spent only a few days of his eventful life in Texas, so David B. Gracy appropriately devoted the preponderance of his study to his subject's life and contribution in other areas. It is impossible to appreciate Austin's contribution to Texas without an understanding of the making of the man in his various business enterprises in Connecticut, Pennsylvania, Virginia, and Missouri. There was formed the character and the mind of Moses Austin, and there was born his drive for success, instinct for survival, and genius for accomplishment.

Gracy begins his story with Richard, the first of the Austin line to move to North America, who emigrated from England in 1638 and settled near Boston. His descendants and their children spread the line south and west as part of the westering experience of America. They were mostly merchants and townspeople. Moses Austin grew to adulthood in Durham, Connecticut, and followed the family tradition. He founded several business partnerships with family members, especially his brother Stephen. When their business expanded, he moved to Philadelphia and then to Richmond, Virginia, to establish other stores. He returned to Philadelphia in 1785 to marry Mary (Maria) Brown. She bore him several children: Stephen Fuller, Emily, and Brown Austin lived to adulthood.

Austin entered the lead business in Virginia and improved production there by importing British lead miners and smelters. Others could have done so, but Austin's keen eye for profit made him the pioneer who dominated the market in Virginia and, later, in Missouri. When the urge to move west to new mines led him into Spanish territory beyond the Mississippi River, he exceeded his achievements in Virginia. In Missouri, he operated Mine a Breton for most of the first two decades of the nineteenth century. Despite constant difficulties in proving and securing title to his

lands, he provided lead sufficient for a nation. He expanded, usually by borrowing, when others reduced their activities. Eventually, his huge debt, the national financial crisis in 1819, and especially the failure of the Bank of St. Louis brought him down, but not before he had founded the cities of Potosi and Herculaneum and attracted more immigrants to Missouri than anyone else.

Amid the crash of his various enterprises, Austin conceived the idea of colonizing Texas. At first, his eldest son offered full support. It was Moses Austin, however, who made the long journey to Texas to obtain permission for a colony in Texas, a colony that Stephen eventually established after his father's death. Austin's death resulted from the exertion of his trip to Texas and efforts to organize his colony and settle his troubled financial affairs in Missouri.

Gracy's greatest achievement lies in keeping the focus of his biography on Moses Austin rather than allowing his study to become an introduction for the achievements of his son. In Texas, Stephen Fuller Austin, the Father of Texas, is much honored. But it is impossible to imagine that Stephen Austin would have established the colony except to fulfill his father's deathbed wish.

This study has a number of aspects that deserve mention. For one, Austin's correspondence is quoted extensively, so readers may draw conclusions for themselves. What emerges from his words is his concern for his family, his determination to be treated as a gentleman, and his confidence in himself. Also, photographs and illustrations are interspersed throughout the text, so readers can visualize persons being discussed immediately.

Stephen F. Austin colonized Texas; Moses Austin contributed to his nation as a businessman, a territorial expansionist, and as a visionary. Those who would understand the founding of Anglo-American life in Texas must first understand Moses Austin, and this is the best account of his accomplishment available.

ARCHIE P. McDONALD
Stephen F. Austin State University

DAVID CRESSY. *Coming Over: Migration and Communication between England and New England in the Seventeenth Century*. New York: Cambridge University Press. 1987. Pp. xiii, 324. Cloth \$39.50, paper \$10.95.

This useful volume provides informative discussions of a wide range of topics concerning the migration to and from New England and the various linkages that bound that region to England during the seventeenth century. These in-

clude the changing character of information about New England available to potential emigrants; the character and motivations of emigrants; the costs of passage and establishing a farm and the means by which poorer people could migrate; the character of metropolitan controls on emigration; the experience of the passage; the economic, social, testamentary, and kinship ties that bound emigrants to those who stayed behind; the extent and nature of the return migration, which during the early decades involved about one in six settlers; transatlantic correspondence as an ongoing "cord of communication" (p. 213); the changing character of metropolitan political news available in New England; and the range and vitality of transatlantic kinship ties.

David Cressy's central concern is to challenge the view, fabricated by contemporary "American Puritan memorialists" (p. 143) and endorsed by many later historians, "that the majority of New England's settlers were driven by religion" (p. 292). Insisting that a "mixture of religious, financial and personal motives" (p. 50) drew people to the region, he reminds us that, "long before anyone imagined it as a place of spiritual regeneration" (p. 6), New England was seen not as a forbidding wilderness, which he correctly characterizes as a later Puritan conceit, but as a fruitful garden, "an appealing alternative" both "to an England that seemed tired, old, overpopulated, and fenced with manorial, parochial and guild restrictions" (p. 11) and to a notoriously unhealthy Virginia. Exhorting his readers to free themselves from "the grip of Puritan legend" (p. 83) and effectively challenging the view of second- and third-generation chroniclers that many of the original settlers were refugees from religious persecution, the author subscribes to the view that the motives of the settlers "had as much to do with private hopes and frustrations, and opportunities of the moment, as with assessments of the comparative religious . . . prospects of England and America" (p. 86). Even among those whose motives were primarily religious, he shows, the decision to emigrate frequently "coincided with a spiritual crisis, a job loss, family problems or debt" (p. 98).

Although Cressy disclaims any intention of trying to substitute a purely secular for the more conventional religious interpretation, he does argue that the "untidy, fractured and complex rather than rational, purposeful and coherent" (p. 74) character of the migration makes the use of terms like Puritan migration and Puritan hegira seriously distorting. At the same time, however, he admits that "religion often provided the language and the frame of reference to explain decisions that involved family, financial and incidental mat-

ters as well as the service of God" (p. 74) and that the New England settlements "were organized and controlled by the godly party" (p. 102), admissions that seem both to qualify the force of this argument and to suggest that the religious dimensions of the early migration may very well have had the defining role attributed to them by contemporary memorialists and later historians.

If the author's major concern is to call attention to the complexity of motives behind the migration, his secondary goal is to put New England in clearer and more accurate perspective in relation to the larger contemporary Atlantic world of which it was a part. Reminding us that New England was only "a sideshow" (p. vii) for the English and that far more emigrants went to other destinations across the Atlantic, he persuasively argues that it is less anachronistic to view "seventeenth-century New England as an outlier of the old country, as a detached English province, than as the seed-bed of a new nation" (p. viii), as a place in which the colonists, at least for the first two generations of settlement, "looked homeward [toward England] rather than towards the American future" (p. 292). His contentions that the region's basic social characteristics—"a literate culture overlaying oral traditionalism, a market economy with an agrarian base, an individualistic and competitive social order in tension with commonweal communalism—had long been part of old England" (p. 292), were present in New England "from the beginning" (p. 293), and cannot therefore be put down as the products of subsequent Anglicization raise important questions about several current historiographical trends. Nevertheless, some historians will continue to believe that the intense religiosity and social communalism of New England and its self-definition as "an informed counterpoint" (p. ix) to old England produced a greater contrast with the parent society than the author suggests.

JACK P. GREENE
Johns Hopkins University

KEITH W. F. STAVELY. *Puritan Legacies: Paradise Lost and the New England Tradition, 1630–1890*. Ithaca: Cornell University Press. 1987. Pp. xiv, 294. \$29.95.

This study is divided into three parts: "Prophetic Strain: The Representation of Puritan Culture in *Paradise Lost*"; "Uneasy Within: The Nature of Puritan Hierarchy"; and "Constant Progress Upward: Puritanism and Capitalism." The book's title is misleading because the work is really three distinct essays, in which connections are forged poorly and the continuities between John Milton's

English Puritanism and the New England Way are only tenuously established. For example, the primary text for examination in the New England sections is the multi-volume *Diary of Ebenezer Parkman*, housed in the American Antiquarian Society in Worcester, Massachusetts, which is a rich resource for social and cultural historical perspectives. Parkman's *Diary* is not, however, representative of New England Puritanism of the eighteenth century (as, for example, the work of Jonathan Edwards would have been), nor does the author make connections between this document and *Paradise Lost* that would reinforce his thesis that there is reason to make a correlation between English and American Puritanism, and "between the English Puritan poet and the American Puritan Commonwealth." Of course, there were connections: many studies have already delineated the analogues between the writings of William Ames and William Perkins in England and the Mather family in New England. John Cotton spanned both worlds, and, since Perry Miller's exhaustive work, we have seen many well-documented treatments of the influence of English Puritanism on the New England Way.

In the present study, however, the legacies or continuities argument is only suggested, not forged, and a few examples of transitional arguments will make this clear. In discussing the seventeenth-century backgrounds, Keith W. F. Stavely argues that "on the eve of his setting forth in leadership of that Great Migration, John Winthrop expressed a sense of moral and spiritual crisis that was almost certainly shared by Milton and his friends" (p. 3). Even the passage from Winthrop's *Journal* that follows as evidence is hardly a convincing association, and its analysis is anachronistic: "What was driving Winthrop and others 'to forsake their dearest home for the savage deserts of America' was that same intense conviction Milton would later articulate in *Comus* and *Lycidas*—the conviction that judgment was about to be passed on an England that had fallen lamentably away from its providential role as an elect, covenanted latter-day Israel" (p. 4). Later, in the long discussions of Ebenezer Frothingham and Ebenezer Parkman, there are some general allusions back to Milton but very little specific evidence of borrowing or even sharing common ideas. "To Ebenezer Frothingham in 1750 it seemed as it had to Milton in *Aeropagitica*" (p. 124), or "As I have suggested, the situation and the impulses . . . seem broadly similar to the moment in Book IX of *Paradise Lost* when Eve insists on working separately from Adam" (p. 137), or, in the opening discussion of Parkman's diary form, the author draws an analogy to John Bunyan's *Grace Abounding*, the fountainhead of Puritan di-

ary literature, but he does so with scant attention to the actual connections between the texts: "Parkman's diary is replete with all aspects of the traditional Puritan sensibility; he was deeply impressed by the sovereignty and omnipotence of God, for example—but what we will primarily examine will be his thoroughly conventional realization that while success in spiritual and moral struggle was utterly beyond his power, given that he was a miserable offender, just such endless struggle was nevertheless his absolute and paramount obligation" (p. 147). This kind of generalizing about continuities among writers, texts, and centuries is insufficient to make the case the author hoped to establish. The result is an uneven, though useful, examination of three centuries and two continents of isolated Puritan documents.

The good news is that the book contains some intelligent perspectives on current historical and critical writing about New England Puritanism. The primary intellectual debts are to Christopher Hill and Raymond Williams, but the book spends too much time rehearsing the contents of the works of scholars such as Perry Miller and Edmund Morgan, both now "classics" in their own right and neither requiring a summary of the meaning of the *New England Mind* or *Visible Saints*. Following these, Darrett Rutman, Richard Bushman, John Demos, Philip Greven, Kenneth Lockridge, Timothy Breen, Philip Gura, David Hall, Paul Lucas, and others are cited as authority on pages 101–36, but there is insufficient evidence from primary texts about "Order and Enthusiasm in New England, 1630–1780," for which there can be no substitute. Rather, we find such generalizations and connections as "Breck's Arminian reasonings were outwardly similar to those Milton had set forth in *Christian Doctrine*" (p. 134). While the writing in this book is clear, and the analysis of patterns of thought, or habits of mind, shared by English and American Puritans is also well presented, the evidence for these connections is tentative, so that the reader is left with these distinctly different little essays in which Puritanism forms the central thematic core. Milton's work is exhaustively assessed but poorly applied to the works that follow it, and it is made to be a center of American Puritan thought in ways that beg more questions than this study answers.

MASON I. LOWANCE, JR.
University of Massachusetts

DAVID W. JORDAN, *Foundations of Representative Government in Maryland, 1632–1715*. New York: Cambridge University Press. 1987. Pp. x, 256. \$29.95.

Aside from Massachusetts, no British North American colony provides a better opportunity

than does Maryland for the study of seventeenth-century social, economic, and political development. At the center of political research has been David W. Jordan, heretofore best known as coauthor with Lois Green Carr of *Maryland's Revolution in Government, 1689–1692* (1974) and coeditor with Edward C. Papenfuss, Alan F. Day, and Gregory Stiverson of *A Biographical Dictionary of the Maryland Legislature, 1635–1789* (1979–84). Based on an effective use of all relevant primary sources and integrating a vast array of secondary works, Jordan's new book is a judicious, scholarly analysis of the evolution of political institutions in His Lordship's Colony that establishes a standard by which similar efforts in other colonies may be judged.

Because Maryland had a proprietary charter based on the medieval Durham palatinate, Jordan argues that there was "no steady 'winning of the initiative'" by Marylanders but rather "an irregular pace of development" often "far removed from the political debates of the mother country" (p. 7). Although one might disagree about how "far removed" the colonists' side of the debate was from Parliamentary arguments of contemporary England, there can be no doubt that there existed two distinct interpretations of the charter provision requiring consultation with the freemen on taxation matters. The proprietor clearly envisioned a version of the circumscribed fourteenth-century Durham assembly, while the colonists sought replication of Parliament's growing powers.

Several factors contributed to the general success of the proprietary forces during the first half-century of colonization. Foremost was the long tenure and political adroitness of Cecilius Calvert (1606–75), second Lord Baltimore and first proprietor. How a Catholic peer kept control of a Protestant-dominated colony in the midst of the Puritan revolution is a matter deserving greater attention than Jordan's assembly-oriented study can provide. What Jordan makes abundantly clear is that any quest for power by the assembly was frustrated by economic deprivation, low population, political inexperience, and internal divisions.

It is ironic that the Restoration of 1660, which seemingly brought greater security to the proprietor's charter, also brought the first major shift toward more representative government. Central to this change were increased economic prosperity, longer-tenured assemblymen, effective assembly leadership, and inept proprietary direction, especially by Charles Calvert (1637–1715), third Lord Baltimore. The overthrow of the proprietorship by Protestant Associators in 1689 led to the

revocation of the charter and the institution of royal government in Maryland.

During the period of royal control, the assembly, "a distinctive social and political elite" (p. 230), secured the privileges of legislative authority that duplicated those in other colonies of the era. Led by wealthy, Protestant, Creole families, the assembly became "the central institution of provincial government" (p. 237). When the charter was restored in 1715, the Calverts made no attempt to return to the Durham assembly model. The way was paved for further concessions to representative government during the remainder of the colonial era.

Most impressive is Jordan's ability to combine into his own political analysis the investigations of new social and economic historians. Thus, the footnotes abound with such names as Lois Green Carr, Lorena Walsh, Russell Menard, and Gloria Main, among others. Although one might question Jordan's often repetitious organization and his assumption of the reader's knowledge of obscure features of Chesapeake history, there is little doubt that this volume will stand the test of time and remain the focus of subsequent studies of early Maryland politics.

DAVID CURTIS SKAGGS

Bowling Green State University

ANNE M. OUSTERHOUT. *A State Divided: Opposition in Pennsylvania to the American Revolution*. (Contributions in American History, number 123.) New York: Greenwood. 1987. Pp. x, 348. \$39.95.

The challenge of explaining the pluralistic societies of the middle American colonies in the eighteenth century has been accepted by a number of astute historians in the last twenty years. Running parallel has been a renewed interest in the loyalists and the reasons for their stands. Anne M. Ousterhout has enriched our understanding of both of these topics in her judicious study. Her focus is on the dissent in Pennsylvania: how it originated, how it was expressed, and how it was treated.

Ousterhout refuses to call these dissenters "loyalists," for she found that most of them did not act out of a sense of loyalty to Great Britain. Rather, she labels them the "disaffected," who acted more on the basis of personal situations and local political attachments than on ideological commitments. Drawing on a wealth of existing studies, the author identifies the ethnic, religious, and group loyalties creating networks that opposed other networks on a variety of issues. Some reasons for disaffection cut across these groups: opportunism, fear of British retaliation, and a conviction that patriot leaders were short-sighted in their dealings with

England. Possibly because of their experience in this pluralistic society, Pennsylvanians were generally tolerant of those who opposed them and treated the disaffected with leniency after the war.

In charting the origins of dissent, Ousterhout merely sketches the complicated political scene before 1765, cautioning that, at the beginning of overt colonial resistance to Great Britain, "there were no Tories and there were no Whigs" (p. 314). It was the later methods of resistance that forced people to take sides. The masterful account of the groups and their interactions from 1760 to 1774 is an unusually clear and concise synthesis. It follows the lead of James H. Hutson's *Pennsylvania Politics, 1746-1770* (1972) in according great importance to the Paxton Boys and the attempt to obtain a royal charter but pays scant attention to the role played by the movement to introduce a colonial Anglican bishopric.

The bulk of the volume is devoted to the period from May 1774 to September 1783, when most of the disaffection arose, was expressed, and was dealt with. Ousterhout leads the reader carefully through this labyrinth, charting the creation and evolution of committees and evaluating the actions, reservations, and motivations of political leaders. She is in general agreement with Richard A. Ryerson, *The Revolution Is Now Begun: The Radical Committees of Philadelphia, 1765-1776* (1978), and expresses any differences with reason and respect. The dilemma of the Quakers, committed both to loyalty to the government and to pacifism, is in the forefront: Germans receive almost no attention.

A section on the frontier provides a graphic case study of the manner in which the troubles with Great Britain were superimposed on, and exacerbated, existing adversarial relations. It is a detailed and careful study of the local problems in the Northeast, where English townships in Connecticut vied with the Scots-Irish in Pennsylvania for control of the Wyoming settlements, and in the Southwest, where Virginians and Pennsylvanians did the same. These conflicts influenced the settlers' relations with the British and Indians during the war.

A State Divided is written with grace, balance, and clarity. Concrete examples and short case studies abound. This topic requires the command of such a volume of secondary literature that one hesitates to quibble about the absence of some important articles and dissertations. Yet surely some work explaining British actions ought to have been consulted in the interest of balance. Likewise, the appearance of the book would have been im-

proved by consistent spacing between lines of type and its usefulness by a more inclusive index.

ELIZABETH I. NYBAKKEN
Mississippi State University

JOHN PHILLIP REID. *The Concept of Liberty in the Age of the American Revolution*. Chicago: University of Chicago Press. 1988. Pp. viii, 224. \$25.95.

Liberty, the "directing force in public affairs" in the eighteenth century, was primarily a "concept of constitutional law." Hence, according to John Phillip Reid, the need for a jurisprudential history grounded in the "taught, familiar language" of the common law. Reid turns aside objections that we must relate an eighteenth-century concept to contemporary discussions, that the lawyer's approach is ahistorical, and that his approach sneaks Whig history in by the back way. Rather, he contends, we have been searching for the eighteenth-century origins of liberty in the wrong places.

The much-maligned term "Whig history" was created by nineteenth-century British historians whose own constitutional tradition was well on the way toward abandoning government "of restrained authority." Parliamentary supremacy, today expressed by an act of will exercised by the party in power in the House of Commons, is a replacement for the Whig theory of law, perhaps even its antithesis. Whig history is simply common-law principles "carried from the legal to the political sphere of argument" (p. 10).

This powerful essay is the most erudite and compact analysis of the vast literature we now enjoy on the fundamental terms and concepts that created American political thought. While making small corrections to the work of Caroline Robbins and Bernard Bailyn, Reid reaffirms the thrust of these historians' labors. He reminds us that the defense of liberty existed in the restraint on arbitrary power, grounded in the ancient constitution, in the struggles with Charles I and in the Glorious Revolution and Act of Settlement. What the history of ideology has overlooked is the centrality of constitutional-jurisprudential thought that had not yet learned to disbelieve in the "rule of law" and, hence, was not a self-conscious ideology. The circuitry of logic about liberty and the rule of law, resting on a pragmatic satisfaction with the current constitution, Reid acknowledges to be rather maddening. Those who seek specific social or cultural ground in which to plant the definition of liberty will find thin soil, for that is not an eighteenth-century way of thinking. It is not, plainly put, historical.

In three swiftly limned chapters, Reid explains how to fix our notions of liberty as the eighteenth-

century theorists did. We need remember that the opposite of liberty was political slavery (arbitrary power), that its "bane" was licentiousness, or an excess of "natural" liberty, and that security in one's property—which included liberty itself and not merely material possessions—provided the three boundaries that set the limits for jurisprudential discussions of the term.

Reid makes no attempt to justify, explain, excuse, or condemn a way of thinking that defined "slavery" without references to chattel slavery. Instead, this tradition insisted on eternal vigilance to guard the female, child-like "blessing" from threats posed by the Catholic French, the Mohammedan Turk, and, most of all, from ignorance, licentiousness, and the "vague and indefinite obedience to the fluctuating and arbitrary will of any superior" (p. 55). Small wonder we have trouble introducing this world to our students, let alone exporting the American revolution.

One may quarrel with the claim that what the Parliament said was law was a notion "not yet accepted in the 1770s" (p. 42). Reid himself identifies from 1716 the beginning of the end for the older tradition of liberty in Britain. Some writers may have been "shocked at discovering that in Great Britain Parliament had displaced law as sovereign" (p. 57), but not enough of them avoided the constitutional crisis that broke in 1775. Reid hints that a broad consensus believing in constitutionalism as the rule of law has preserved more of eighteenth-century liberty in the United States than in Britain itself.

This is a splendid little essay and indispensable reading for any serious student of the eighteenth century.

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IAN DYCK, editor. *Citizen of the World: Essays on Thomas Paine*. New York: St. Martin's. 1988. Pp. viii, 152. \$29.95.

This slim volume is a memorial to George Spater, lawyer and former chief executive officer of American Airlines but best known to historians as author of the admired two-volume biography of William Cobbett. Five of the eight essays are by Spater, drawn from material he left behind after his sudden death in 1984. His first four add little to what knowledgeable admirers already know about Paine, but the fifth, titled "The Legacy of Thomas Paine," indicates the superb book he would have produced had he been given time. In

this final essay Spater makes a case for Paine as citizen of the world; he holds that the shift since the eighteenth century from minority to majority rule among Western nations "is without doubt the greatest political change in recorded history" and that "Paine must forever take first rank among those who precipitated the shift" (p. 135).

Spater did not plan to write another biography of Paine. Instead, he wanted to study the impact of Paine's thought on Western civilization during the last two centuries. The diverse essays by the three other contributors are tied together by this theme. Each is competent, and collectively they drive home a point that Spater did not live long enough to develop fully. J. F. C. Harrison writes about Paine's influence on millenarian radicalism, Ian Dyck on Paine and Cobbett, and Joel H. Weiner on Paine and Richard Carlile. They treat Paine with dignity, not once denigrating him with the nickname "Tom," as most academics, notably Americans, do. All seek to rectify the neglect he has suffered from scholars. This neglect is puzzling. All of Paine's important works remain in print some two centuries after they were published, a rarity for an eighteenth-century writer, and they continue to sell well in numerous paperback editions. Yet despite his popularity the two hundred fiftieth anniversary of his birth passed virtually unnoticed in 1987. A small band of admirers who tried to get the U.S. Post Office to mark the event with a stamp honoring Paine were told that their candidate lost out to another transplanted Englishman, Stan Laurel, and his partner, Oliver Hardy.

"Citizen of the world" is an apt title for Paine, for so he regarded himself. But the little solid scholarly work done on the man has focused on his influence in Great Britain. The essays by Harrison, Dyck, and Weiner continue this pattern, to a large extent amplifying insights found in E. P. Thompson's *The Making of the English Working Class* (1963). Spater's essay on the legacy of Paine suggests that Spater would have redressed the balance by emphasizing Paine's influence in America, especially among the immigrants who poured into the country during the nineteenth century. Perhaps Spater might also, if he had lived, have managed to erase from the public mind that ill-informed but trenchant phrase of Theodore Roosevelt, Paine, that "filthy little atheist."

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JERRILYN GREENE MARSTON. *King and Congress: The Transfer of Political Legitimacy, 1774–1776*. Prince-

ton: Princeton University Press. 1987. Pp. xiii, 462. \$29.95.

This is an old-fashioned book. In form it is a political narrative, written from the top down. In content it presents a strong argument against the prevailing view of the American revolution, which holds that it was profoundly revolutionary. What Jerrilyn Greene Marston finds instead is profound continuity. But the book is more than a reassertion of an earlier generation's methods and conclusions. If its large thesis does not convince, it is not for want of massive research. Nor is it for want of sophisticated thought.

A lawyer as well as a historian, Marston writes from a conservative stance. Her argument is that Congress rapidly took on the qualities of a national government and that the result was to spare the United States most of what has happened in other revolutions. She understands Congress as an executive body, taking the crown as its model, assuming one royal power after another, re-creating what had gone before, innovating only with reluctance. Thus, George Washington, Congress's agent, transformed the potentially radical army that assembled after Lexington and Concord into a European-style force. The result of such developments was to turn Congress into "a true continental government . . . as close a replica as circumstances would allow of the old imperial executive" (p. 299).

Marston echoes the scholarship of Jack N. Rakove (*The Beginnings of National Politics* [1979]) and Richard B. Morris (*The Forging of the Union, 1781–1789* [1987]) in giving primacy to events at the center. She challenges Gordon S. Wood's assertion in *The Creation of the American Republic, 1776–1787* (1969) that in 1776 real political excitement accompanied the making of the state constitutions. She finds instead that state leaders were reluctant to act until Congress gave a lead and that "they established governments that . . . differed little from the vacating royal governments they replaced" (p. 296). Pennsylvania's extreme democracy ceases to define the limits of radicalism, exemplifying what some people elsewhere also wanted, and becomes a mere exception to her general rule.

This book is the most stimulating account of the early years of Congress I have read. It concentrates—correctly, I think—on the problem of the transfer and exercise of power. It forces one to think afresh about relatively small events (such as John Dickinson's Olive Branch Petition) and about large ones (such as the creation and nature of the Continental Army). It never makes an assertion without the support of a mass of evidence. In

short, as a treatment of the first years of Congress, it has no rival.

But it does have its limits. One cannot fault Marston for not writing a different book, but it is still the case that she loses most of what the past generation of scholarship has gained. Clashing social perspectives, the influence on events of different sorts of people, the impact of the events on the people themselves, and the opening of exhilarating new possibilities are missed by Marston except to the extent that they posed a problem for the emergent white male national elite to solve. We can no longer regard the revolution as the sole property of such people, as Marston tends to do. Nonetheless, she has produced a monograph of considerable merit, one that deserves thoughtful study.

EDWARD COUNTRYMAN
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DANIEL T. RODGERS. *Contested Truths: Keywords in American Politics since Independence*. New York: Basic Books. 1987. Pp. x, 270. \$19.95.

"If you want to know what a politician is up to watch his feet, not his mouth." Thus Marvin Meyers (*The Jacksonian Persuasion* [1957], p. v) captured an uncontested truth from the generation of American historians reared on Charles Beard. Fifteen years later, reviewing two books in the *Journal of Interdisciplinary History*, J. G. A. Pocock explained that men "cannot do what they have no means of saying they have done; and what they do must in part be what they can say and conceive that it is." Between those two statements lies a revolution in our collective sensibility as historians. Just to ponder them as texts gives a feeling for the change. Meyers wrote like politicians spoke, Pocock like philosophers think.

Daniel T. Rodgers's book appears to reflect this revaluation of what historians consider significant but without showing signs of having been influenced by the sociologists of knowledge, the cultural anthropologists, and the literary theorists who have inspired this revision. There is little here about discourse, textuality, and paradigms, particularly paradigms. Rodgers is quite explicit in rejecting the Kuhnian approach that has enraptured his fellow historians. He does not believe in paradigms or their shifts. Intellectual traditions in his judgment "are the convenient inventions of intellectual historians" (p. 47). Although "keywords" appears in his subtitle and he acknowledges its provenance in Raymond Williams's study (p. 227, n. 5), he has not provided an American glossary of politically resonant terms; rather, he has concentrated on the circumlocutions of speech from

American political figures, choosing seven terms to follow: utility, natural rights, the people, government, the state, interests, and freedom.

Devoting a chapter to each, Rodgers organizes his material sequentially, carrying his study from the Declaration of Independence to the New Deal with an epilogue on recent usage. His account begins with the puzzling failure of Jeremy Bentham's utilitarianism to catch fire among his contemporaries in the United States. This otherwise English-centered chapter enables him to introduce the paradoxical love of abstractions among a people noted for being practical. Rodgers then traces a retreat from talk of natural rights in the 1820s, a retreat he finds camouflaged by the making of popular sovereignty into slogan. The linguistic focus then shifts to the keyword of "government" infused in the antebellum period with the proprietorial moralism of Protestant speakers. After the Civil War, it is the turn of professionals—judges and political scientists—to dominate the language of politics with their preoccupation with the state. Rodgers concludes his study with an examination of the New Deal lexicon of interests. Politics have capitulated at last to the conceptual universe of the market. In his epilogue Rodgers reviews the two-century sweep he has followed, noting soberly how words such as freedom, which have empowered individuals, have regularly been spoken while Americans have had difficulty giving voice to concerns about the public good.

It is understandable why Rodgers wished to write a book about words because he uses them very well. More than that, he is sensitive to words—to how they entangle their users in embarrassing corollaries or are manipulated to distract attention from seductive meanings. Less successful is his effort to make incisive his theses about "contested truths." He asserts the existence of conflicts over words (pp. 46, 11, 147, 176) but never elucidates these verbal contests. Indeed, in many places, assertions about contested words merge imperceptibly into discussions of contests over actions or goals. Methodologically the book breaks no new ground, nor does the organizing device of keywords really work. Rodgers is the omniscient author, explaining in a conventional way actions with evidence from texts and texts with evidence from action. Yet he has many interesting things to say, especially about the writing and rewriting of constitutions in the United States. If I were to state the unworded thesis of the book, it would be the following: since 1776 the strongest rhetorical movements in American politics have been designed to lead voters away from the radical implications of the Declaration of Independence. This diversionary effort has turned American pol-

iticians into wordsmiths who are forever trying to blunt with public oratory the cutting edge of the nation's revolutionary inheritance. I find this a fascinating idea.

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JOHN CORRIGAN. *The Hidden Balance: Religion and the Social Theories of Charles Chauncy and Jonathan Mayhew*. New York: Cambridge University Press. 1987. Pp. xiv, 161. \$29.95.

According to John Corrigan, "twentieth-century historians often have trouble understanding the eighteenth-century belief that cosmic order was based upon a principle of dialectics that could never be fully grasped by the human mind" (p. 146). This has particularly been the case, he argues in this volume, with Charles Chauncy and Jonathan Mayhew, the two ministers of revolutionary Boston whose printed sermons have been so widely studied by historians of the period. Those scholars who have approached this provocative pair with separate interests in rational religion, political philosophy, and colonial social structure have missed "the hidden balance" of Chauncy's and Mayhew's thought, their "attempt to integrate ideas in all of these areas into a coherent ideology" (p. 112).

Chauncy and his younger ministerial colleague found that coherence in the works of the European Enlightenment, the English rationalists, and particularly in the concept of the Great Chain of Being, by which they understood that all parts of the cosmic order were linked in a unified whole, perfectly understood only by the Creator. Thus, there could be no separation of religion, society, and government. This understanding of harmony in the plan of God guided the minds of these two pastors who lived in an in-between world: between the Puritan holy commonwealth and the growth of individual wealth, between religion of the heart and of the head, and between political liberty and loyalty to a sovereign. Like the porch on the strikingly beautiful jacket of this book—neither in the house nor outside it—Chauncy and Mayhew were "neither strict traditionalists nor bold prophets. They were, simply, in between" (p. 2).

This stimulating essay in intellectual history has chapters on the two preachers' views on religion, government, and society, all attempting to show the same "hidden balance" that gave coherence to the views expressed in their oft-quoted writings. It is unfortunate that Corrigan attempted to tie their social philosophy to the composition of their congregations with an economic profile of each body.

Although Mayhew's West Church parishioners were often young and enterprising, Corrigan's evidence for the "high status" of the members of Chauncy's First Church is unconvincing. When compared to other Congregational churches in Boston, those under Chauncy's ministry appear to have been no better than average or below average in wealth and social status. Corrigan could have profited from the study of Boston merchants in John W. Tyler's *Smugglers and Patriots* (1986) or from examination of the Brattle Street congregation, in which the town's mercantile wealth was heavily concentrated. Instead of doing the comparative study of Boston churches necessary to prove his contention that these two congregations were "unusually rich," Corrigan appears to have assumed that the social theory of their pastors must have had as a base members of "high status."

This book is well worth the attention of scholars for its closer examination of the ideas of Chauncy and Mayhew, but it also illustrates the pitfalls of trying to integrate intellectual and social history.

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RICHARD W. POINTER. *Protestant Pluralism and the New York Experience: A Study of Eighteenth-Century Religious Diversity*. (Religion in North America.) Bloomington: Indiana University Press. 1988. Pp. xv, 205. \$25.00.

New York has long annoyed religious historians. Settled by a variety of peoples who, unlike the Puritans, did not record every waking thought, complicated by a heterogeneity that included Dutch and German congregations producing Dutch and German records, New York's religiosity has been almost unapproachable. Yet Richard W. Pointer moves confidently onto this territory. He explores various denominations across the eighteenth century and discovers a culturally pluralistic society that was both increasingly tolerant and spiritually alive.

In six essays, Pointer identifies a primary characteristic of New York's religious environment, pluralism, and traces this pluralism in terms of three themes: institutional structure, church-state relations, and spiritual vitality. New York's diversity, he argues, generally strengthened Protestant congregations through denominational competition. Many groups constructed autonomous church organizations able to withstand the pressures of the revolutionary era. Following the war, shared space and shared crises opened the way for competing congregations to cooperate for their mutual prosperity and the greater glory of the gospel. While providing such reciprocal benefits

and understanding, this pluralism also encouraged widespread support for religious freedom and disestablishment. Anxious for its own liberty, each denomination accepted the price: liberty for all—including Catholics, Jews, and free thinkers—and the establishment of none.

In these discussions Pointer is at his best. With a logical argument well supported by his sources, he argues that pluralism buttressed stability and promoted religious equality. Pointer's primary weakness here is his dependence on secondary works for both data and interpretations that all too often stand unquestioned. The very complexity of New York's religious environment, however, obviously requires research gleaned from other scholars, and Pointer's arguments are carefully constructed from this mixture of sources.

Pointer's third theme, spiritual vitality, is the most promising and the least rewarding. That pluralism would strengthen some believers and confuse others seems obvious; that the Great Awakening split some congregations while cutting across denominational lines has long been established. More fruitful material appears in the sixth essay on the Second Great Awakening. Here Pointer convincingly demonstrates that New York's revivalism was qualitatively different from that experienced in the West and that the New York awakening signified spiritual health, not decline.

Unfortunately, the discussion of popular religion ends here. Although Pointer wants to know how pluralism affected the individual's religious experience, his institutional sources and traditional models cannot by themselves discover those connections. I wish he had delved more deeply into popular religion, for his impressive command of the various denominations would have provided a sure foundation for such speculation. For example, he missed a sterling opportunity to tie together the First and Second Awakenings in terms of popular participation and experience. As it stands, his analysis remains limited by a focus on the traditional topics of institutions, clergy, and government. Be that as it may, Pointer has written a fine piece of church history that explores the interactions of denominations and politics in eighteenth-century New York. He has filled an important gap in the religious history of colonial America.

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RANDOLPH A. ROTH. *The Democratic Dilemma: Religion, Reform, and the Social Order in the Connecticut River Valley of Vermont, 1791–1850*. New York:

Cambridge University Press. 1987. Pp. x, 399. \$34.50.

This book is easy to admire but hard to enjoy. Randolph A. Roth shows us many parts of the complex cultural patchwork of postrevolutionary Vermont—Old Lights, New Lights, Republicans, Federalists, freethinkers, teetotalers, Antimasons, abolitionists, revivalists, and reformers of all sorts. Then he displays the whole piece, an intelligent interpretation of the nature of democratic society that covers far more than Vermont. Clearly, it is an ambitious work made of rich material. Unfortunately, Roth makes it all seem rather colorless. Intriguing hints of human detail are often obscured by bland generalizations written in lifeless, long-winded prose. Some sentences seem endless, strung out with so many clauses, modifiers, and qualifiers that the reader looks longingly for the end—and then has to go back and unravel several lines to find the meaning.

Those who have the patience for such work—specialists but probably not most students—will be rewarded with an interesting perspective on post-revolutionary society. The “democratic dilemma” stems from the essential tension between republicanism and Calvinism, the difficulty of creating a “tolerant and equitable yet orderly and pious society” (p. 115). Unlike their neighbors in southern New England, Vermonters did not establish a standing order, although some certainly tried. By the early nineteenth century, it was clear that Vermont’s diversity defied any sort of coercive control. Yet it was also clear to many Vermonters, especially local elites in the communities of the Connecticut River valley, that they needed (or wanted) some sort of stable order to regulate the divisive tendencies of democracy. Moreover, periods of economic decline frustrated individual and familial ambition, adding to people’s sense of uneasiness. Some Vermonters looked for relief to “insurgent” political movements—the Jacksonian, Antimasonic, or Workingmen’s parties—and many more found hope in reform movements and religious revivals. All of these movements created their own sources of division, however, and none was able to unite a majority of Vermonters under a single banner. Yet, despite the divisions, local leaders eventually helped fashion a fairly stable, or “modified,” order; they drew on the basic impulse of reform sentiment but emphasized not so much perfectibility as respectability. By the middle of the century, Roth argues, Vermonters considered themselves the “creators of the most Christian and democratic society on earth,” and they believed that “they had been chosen to be the custodians of America’s (and indeed the world’s) moral, spiritual, and political heritage” (p. 14).

That is quite a large responsibility for a small state to bear and quite a large argument for this book to bear. One wonders if Roth has perhaps magnified the importance and the intensity of Vermont’s struggle for order. In his discussion of political insurgency, for instance, Roth writes that the Workingmen’s party made a “strident (albeit moderated) attack on the postwar order” (p. 161). Then, two pages later, he concludes that “their program boiled down to little more than a call for moral reawakening” (p. 163). In this, as in many other cases, Roth’s emphasis is on strident conflict, but his evidence suggests a more moderate resolution. Yet one also wonders if the moderate resolution of “modified” reform order was as readily accepted as Roth suggests. By the middle of the nineteenth century, ethnic and class distinctions only increased the divisions among Vermont’s people. Some Vermonters may well have committed themselves to remaining “the most dedicated Christians and reformers on earth” (p. 310), but others no doubt had different ideas about their most immediate interests. Like the nineteenth-century reformers of rural Vermont, Roth has a clear vision of what he wants his society to look like, but ultimately he, too, has trouble keeping everyone in the same picture.

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RICHARD NELSON CURRENT. *Arguing with Historians: Essays on the Historical and the Unhistorical*. Middletown, Conn.: Wesleyan University Press. 1987. Pp. 208. \$19.95.

“Believe it or not,” writes Richard Nelson Current with astonishment, “the Civil War came because Abraham Lincoln suffered from an Oedipus complex” (p. 52). The recent “psychologizers” (as he calls them) who have stripped Lincoln down to his psyche in order to expose his unconscious wishes and motivations are only a few of those with whom Current argues in this instructive and diverting collection of essays. In the eleven studies he has gathered (all but one published elsewhere), he examines some of the principal historiographic trends of the past several decades, focusing on the themes of the Civil War era: nationalism, sectionalism, and relations between blacks and whites. Within this range Current takes on those who insist on the relevance of John C. Calhoun’s political philosophy to our own day; recent biographers of Lincoln who have shaped Lincoln’s ideas to serve present causes; historians of Reconstruction who have distorted Thaddeus Stevens’s motives, overlooked Ulysses S. Grant’s significance to what Current calls the “continuing civil war,” and

furthered the negative stereotype of the carpet-baggers; novelists who disguise fiction as history; and the supporters of the "new ethnicity" as a key to an understanding of the past. Only the volume's initial essay, in which Current persuasively refutes the arguments for the authenticity of the Mecklenburg Declaration, stands outside the Civil War period. Current does not claim the last word on these issues—"History, after all . . . is an ongoing argument about the meaning of the past" (p. 8)—but he does concede that he may have put some of them to a long overdue and well-deserved rest (Calhoun's relevance, the Mecklenburg Declaration, the question of where responsibility lies for the Fort Sumter crisis).

There is no mistaking which trends Current regards as "unhistorical." Among older interpretations, he singles out Civil War revisionism (unfairly labeled as "pro-Confederate") and the prosouthern, antiblack Dunning school of Reconstruction historiography (from which he exempts James W. Garner's *Reconstruction in Mississippi* [1901] as being remarkably accurate and objective for its time). Current, however, reserves his major attack for more recent historiographic directions, targeting especially those who engage in guesswork and mind reading at the expense of historical objectivity and who thus merely replace old myths with new ones. Among these new myth makers, he has least patience for Lincoln's psychobiographers. He takes strong exception to Erik Erikson's brief (adopted by the psychobiographers) in behalf of "half-history," a mode that does not contradict known facts, has the "ring of truth," and is consistent with psychological theory. Although he finds some redeeming qualities in their work (Charles Strozier's, for example), Current rejects their premises and dismisses their conclusions as little more than "denigrating myth." To "pry into motives" (p. 86), he cautions, is dangerous, although he comes close to doing that in his essay on Stevens. If inferences are to be made from words uttered in the past, Current advises, they must be in keeping with the context of the time.

Equally culpable are those who "rewrite the past to serve a present cause," specifically those who have tried to enlist Lincoln in the modern civil rights movement. Their efforts are "in the very essence of myth making" (p. 70). So also is the "fictional history," which Current carefully distinguishes from "historical fiction," of Gore Vidal, Alex Haley, and William Styron, whose works have revealed at worst a woeful disregard for the truth of history and at best a misreading of the past. Confronting the great popularity of these works, Current despairs: "Past reality and present fantasy blend more and more into an inseparable

mix. Apparently few people care except us historians—and not even all of us" (p. 161).

There is much to be gained from this collection, not only the insights it offers into some of the lingering problems of Civil War and Reconstruction historiography but also its cautionary message. Historiography, Current believes, must inevitably reflect the "spirit of its own times" (p. 162). Yet the traditional standards of historical truth and objectivity must remain constant. Against these standards, new and attractive methodologies that promise quick and easy enlightenment must be measured with unrelenting thoroughness. The question, disarmingly simple but nonetheless extremely important, is "whether the predilections now fashionable are likely to lead to clarity—or to distortion—in our understanding of the past" (p. 162).

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SAMUEL L. HORST. *Education for Manhood: The Education of Blacks in Virginia during the Civil War*. Lanham, Md.: University Press of America. 1987. Pp. viii, 292. Cloth \$23.25, paper \$14.75.

University Press of America has once again burdened scholarship with a volume homely in visual presentation, lugubrious in prose, and sophomoric in editing. Those faults may be bearable when the contents reward the perseverant. Unfortunately, this book offers few rewards.

Samuel L. Horst has completed a labor of love. The research is exhaustive and exacting. The result of his labor, however, is merely exhausting. In tortuous detail, more akin to badly written military history than to the social history usual to the subject matter, the volume chronicles the efforts of northern whites to educate the freedmen in Virginia and the District of Columbia over a mere four years. He minutely follows the movements of teachers and superintendents across that limited chronological and geographical landscape, seldom with benefit to the book's larger concerns or to scholarship more generally. The educators' actions were conservative, he argues, and were molded by their assumptions about slavery.

From start to finish, Horst is insistent that slavery was merely an "alleged evil," that Afro-Americans suffered only "presumed degradation" from their experience with slavery. This interpretive perversity apparently results from his desire to distance himself from Stanley Elkins's heresy. The result, however, is a stance that resurrects the assumptions of southern apologists and that implies that forms of political, economic, and social life are unimportant, wielding insignificant influence on those living under them.

Underneath that stance run his central arguments: that freedmen's educators sought to limit social change through schooling and were victims of their own assumptions about slaves and slavery. Those arguments seem no longer to have detractors; other questions of the ideological sources of those problems, and their consequences for blacks and whites, now dominate scholarship. Yet Horst is unmindful of current scholarly work. Of the wealth of books, articles, and dissertations on freedmen's education produced in the last decade, he cites but two minor pieces. Meanwhile, Henry L. Swint's *The Northern Teacher in the South, 1862-1870* (1941), with all its Dunningite racial and regional baggage, is the only focused study cited.

In the Swint tradition, Afro-Americans are silent in this volume. They appear here only in the far background, dimly perceived, a mute mass acted on but never acting, the objects of competing white visions but never the subjects of their own visions. And thereby, Horst, himself a man of progressive political commitments, offers a study in full political, social, and historiographical retreat.

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WILLIAM L. BURTON. *Melting Pot Soldiers: The Union's Ethnic Regiments*. Ames: Iowa State University Press. 1988. Pp. x, 282.

For scholars of ethnic history and ethnicity, there is still a controversy as to whether the Civil War signaled the final Americanization of the ethnic groups involved or whether it retarded the process of assimilation. William L. Burton's readable and well-illustrated book suggests, as in its main title, Americanization.

Because Burton aptly places the ethnic regiments in the broader context of state and local politics, defining them as an "outgrowth" of those structures (p. ix), he dedicates the first chapters of his book to ethnic politics, recruitment policies, and the general political environment. The author delineates the motivations of those from ethnic groups who volunteered for the Union army and successfully demonstrates the complexities of this decision process. Political idealism, a yearning for adventure, and economic reasons mingled together, reflecting the specific environment of each ethnic group. The problem Burton faces here is how to avoid repeating information already familiar to the scholar of ethnic history without jeopardizing the dialectic of his argument. Almost inevitably omissions and shortcomings result. There are some mistakes: for example, the radical Ger-

man Forty-Eighter journalist Carl Heinzen lived and published not in New York but in Boston during the Civil War (p. 88). Also Burton's trivialization of the role of nativism is misleading; the nativistic crusade of the 1850s was indeed a traumatic experience for the Irish- and the German-Americans, and German-Americans did not possess, as Burton argues, a "secure and comfortable identity" (p. 7) in the decade before the Civil War.

In the successive chapters, regiments are described according to their ethnic composition, and Burton concentrates on leadership and the particular involvement of these troops in the political and military aspects of the war. Interestingly, Burton does not deliver a static picture of the regiments but traces them throughout the conflict and rightly states that "change over time . . . tell[s] us much about the role of ethnics in America" (p. ix). Unfortunately, nothing is said about the sociological profile of these units, which would enable us to use Burton's findings for a comparative study of other regiments.

The book closes with general reflections on ethnicity, ethnic self-perception, reaction by society, rivalries within the ethnic community, and forces of assimilation. In contrast to the author's concept of the Civil War as a melting pot, many sources suggest that a sense of separateness continued throughout the conflict. While investigating the main issues related to this subject, Burton certainly should have consulted foreign-language newspapers published in the United States and made greater use of the papers of individual officers and soldiers, for example, the Franz Sigel manuscript collection at the New-York Historical Society, which is one of the richest sources of information on an ethnic Civil War leader. These documents tell us much about ethnic self-perception, ideology, motivation, and change over time. A deeper cross-cultural comparison and analysis of the different ethnic regiments would have enriched the concluding chapter.

Despite these caveats, Burton's book—which, unfortunately, does not supersede Ella Lonn's classic but dated *Foreigners in the Union Army and Navy* (1951)—is a contribution to a better understanding of ethnicity, assimilation, and acculturation during the Civil War. It will, one hopes, stimulate further research, based on a multidimensional approach, on this important subject.

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JAMES LEE MCDONOUGH and JAMES PICKETT JONES. *War So Terrible: Sherman and Atlanta*. New York: W. W. Norton. 1987. Pp. xx, 385. \$19.95.

Prior to 1864 American Civil War armies maneuvered toward each other, fought a battle not exceeding three days in length, then separated for a period of weeks to lick their wounds and leisurely prepare for the next encounter. That style of warfare changed with the arrival in supreme command of Lieutenant General Ulysses S. Grant. Grant envisioned changing the tempo of combat by using the North's superior resources to maintain continuous pressure on the dwindling but still dangerous southern armies. While he moved with the Army of the Potomac in the East, Grant entrusted the task of defeating Joseph E. Johnston's Confederate Army of Tennessee to his friend William T. Sherman. Grant expected Sherman to focus on the opposing army rather than a specific place, but Sherman's campaign inexorably moved southward toward the city of Atlanta, Georgia. Although Atlanta contained some factories that contributed to the Confederate war effort, its primary importance was that of transportation hub and distribution point. Railroad connections made Atlanta the "turntable of the Confederacy." As such, the city eventually replaced Johnston's army as Sherman's goal.

An example of relentless offensive pressure sustained by a massive railroad-based logistical effort, the Atlanta campaign resembled campaigns of the future more than it did campaigns of previous wars. Terrain and the size of the opposing armies provided fascinating opportunities for commanders to use maneuver to affect the outcome. In addition the campaign represents an excellent case study of the intangible factors so important in war, such as troop morale and the role of personality among senior officers. The campaign's importance notwithstanding, it has received surprisingly little book-length treatment. Save for participant Jacob Cox's *Atlanta* and Samuel Carter's popular account, the campaign can only be studied piecemeal through official documents, memoirs of participants, biographies of major figures, unpublished dissertations, and numerous articles. A comprehensive analytical study of the entire campaign has long been needed.

Unfortunately, the present book by James Lee McDonough and James Pickett Jones goes only part way toward meeting the need. McDonough and Jones have produced a well-written summary of the principal events of the campaign, but their emphasis on spritely narrative has led them to sacrifice analysis for style to an unfortunate degree. They rightly argue that the Federal side of the campaign has been examined less than the Confederate, but they do little to rectify that omission by probing deeply into the causes of events. Sherman's occasionally erratic performance and the strains among the components of

his army especially merit closer attention than they receive. Even the Confederate analysis presented rests primarily on the earlier work of Thomas Connelly and Richard McMurry rather than on the considerable analytical skills of the authors. Key analytical questions, beginning with the difficult task of assessing Confederate numbers, are either dodged or covered so quickly that their impact is often lost. The authors do not provide any footnotes so that their sources may be readily traced, a serious flaw not mitigated by the generally good bibliographical essay. Finally, both text and maps are often marred by obvious errors. For example A. S. Johnston was not a Virginian (p. 27), the railroad does not divide at Adairsville (p. 130), Grenville Dodge was in the Army of Tennessee (p. 232), twenty-pound howitzers are fictitious (p. 231), and there was only one major trestle in Running Water Canyon (p. 330). Separately, these and other errors seem minor; collectively, they are troubling and bespeak an unseemly haste into print. In sum, narrative appeal for the popular market has triumphed over scholarship. Sadly, the Atlanta campaign still awaits its analytical due.

WILLIAM GLENN ROBERTSON

U.S. Army Command and General Staff College
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BENJAMIN FRANKLIN COOLING, *Forts Henry and Donelson: The Key to the Confederate Heartland*. Knoxville: University of Tennessee Press, 1987. Pp. xiv, 354. \$24.95.

In February 1862, northern naval and military forces commanded by Captain Andrew Hull Foote and Brigadier General Ulysses S. Grant captured Fort Henry on the lower Tennessee River and nearby Fort Donelson on the lower Cumberland River. Union possession of these two strongholds broke the Confederacy's grip on West Tennessee and opened the way for the Federals to wrest control of the area between the Appalachian Mountains and the Mississippi River from the rebels. Some historians have regarded the Henry-Donelson Campaign (with its aftermath—the April 1862 battle of Shiloh, which demonstrated that the southerners could not undo the damage they had suffered when they lost the forts) as the most important military engagement of the American Civil War.

Surprisingly, in view of its undoubted importance, the struggle for the forts has not heretofore received much detailed attention from students of the war. Now Benjamin Franklin Cooling, who once worked as the historian at Fort Donelson National Military Park, has provided us with a fine account of the campaign that, among other things,

"broke open the war in the West" (p. 245) and first brought Grant into prominence.

Cooling's work is based on an impressive list of sources, and it provides the reader with far more than a simple narrative of the events of early 1862. The background material on the West Tennessee area and the concluding three chapters ("A Season of Lost Opportunities," "The Aftermath of Donelson," and "Henry-Donelson Remembered") are especially interesting. Cooling also does a fine job of delving into the political-military-bureaucratic squabbles that sometimes played as important a role in a campaign's outcome (for both sides) as did the more obvious military factors.

Some of Cooling's word usages—"not only . . . but" when he clearly means "not only . . . but also" (pp. 12, 65, and elsewhere); "10 A.M. that morning" (p. 28); artillery shells "impacting in the . . . area" (p. 195); and the use of plural pronouns with singular antecedents (p. 255 and elsewhere)—will trouble purists of the old school (including this reviewer). All readers, however, will find Cooling's book to be the best general account of the fight for Forts Henry and Donelson that we are likely ever to have.

RICHARD M. MCMURRY
North Carolina State University

WILLIAM GARRETT PISTON. *Lee's Tarnished Lieutenant: James Longstreet and His Place in Southern History*. Athens: University of Georgia Press. 1987. Pp. xv, 252. \$24.95.

This book is a bold analysis of James Longstreet's Civil War military career and place in southern history. William Garrett Piston, one of Thomas L. Connelly's students, is concerned more with tracing Longstreet's "fall from grace" than with writing a full biography. He concentrates first on his subject's "contribution to the Confederate war effort" and then on "the creation, manipulation, and persistence of Longstreet's image" (pp. x-xi).

Appointed a Confederate brigadier general in July 1861, Longstreet rose rapidly in rank to lieutenant general. He served in the eastern theater first under P. G. T. Beauregard, then under Joseph E. Johnston, and finally under Robert E. Lee, who entrusted to him "the greater portion of his troops" and made him his "chief lieutenant and second in command" (p. 30). Despite rough manners and outspoken opinions, Longstreet gained the respect and admiration of most of his subordinates.

After engaging in nearly all of the major eastern campaigns from First Manassas through Gettysburg, he took his corps west to reinforce Braxton Bragg's Army of Tennessee. At Chickamauga his

men crashed through the Union line and won for him "the greatest achievement of his career" (p. 72). A disappointing independent command in eastern Tennessee followed his success in Georgia, and he ultimately returned to corps command in Lee's army.

Longstreet enjoyed a "favorable and creditable" (p. 99) image for a time following the war, but white southerners turned against him after he joined the Republicans. Today he is "remembered primarily as Lee's tarnished lieutenant" (p. 188). Piston is sympathetic but not uncritical in tracing the transformation of Longstreet from hero to villain. He admires Longstreet's skills: "his ability to move large numbers of men without confusion" (p. 17); his "magnificent" courage, tenacity, and resolution (p. 25); and his "remarkable physical stamina [that] allowed him to make personally the sort of meticulous observations that were beyond a man of Lee's age and indifferent health" (p. 36). Believing that "probably no other officer in the South could have taken control as quickly and smoothly or with such self-confidence as Longstreet at Chickamauga" (p. 70), Piston emphasizes Longstreet's strong commitment to defensive tactics. Yet he admits that Longstreet, who had "significant limitations" (p. 81), made mistakes. Piston, like Connelly, is hard on the Virginians, and he is especially critical of Lee, emphasizing his "uneasiness" (p. 53) and describing him as "jittery" (p. 64) at Gettysburg. Such perspectives, along with the book's readable style and concise battle descriptions, excite interest.

Parts of Longstreet's career are treated less satisfactorily. Bits and pieces fail to fit; nagging questions remain. Downplaying the ambition of Longstreet, Piston ignores much of the guile and flattery in his letters, seems at times to exaggerate his humility, and speculates on what Longstreet thought without offering sufficient supporting evidence. His actions at Gettysburg still remain uncertain, although Piston tries hard to exonerate him. In advocating a western strategy, Longstreet does not appear to have been the selfless soldier that Piston depicts. How, for instance, can a man who repeatedly wanted to swap his corps for command of an army, as Longstreet did, appear less than egotistical and consumingly ambitious?

Piston doubtless is correct in portraying Longstreet as his own worst enemy in the postbellum South. His politics made him an easy scapegoat. During Reconstruction he may have been "Machiavellian" and "misunderstood" (p. 107), but in a New Orleans riot Longstreet led black police against Confederate veterans. Defenders of the Lost Cause saw such collaboration with the enemy as nothing less than betrayal.

The author of this lively, controversial, and

significant book deserves applause. His fresh perspective raises many questions regarding Longstreet and his contemporaries.

GRADY MCWHINEY
Texas Christian University

ELI N. EVANS. *Judah P. Benjamin: The Jewish Confederate*. New York: Free Press of Macmillan. 1988. Pp. xxi, 469. \$24.95.

Judah P. Benjamin, U.S. senator from Louisiana (1853–61), occupant of three cabinet positions in the Confederate government (attorney general, secretary of war, and secretary of state, in turn), and professionally involved with President Jefferson Davis on an almost daily basis during the war years, made life difficult for his biographers: he destroyed his private papers. Davis further complicated matters in 1881 when, in a fifteen-hundred-page tome, *The Rise and Fall of the Confederacy*, he made only two minor references to Benjamin. Thus, Eli N. Evans, in reconstructing the life of this “Jewish Confederate,” has been forced to rely primarily on the writings and manuscript collections of Benjamin’s two previous biographers, Pierce Butler (*Judah P. Benjamin* [1907]) and Robert D. Meade (*Judah P. Benjamin: Confederate Statesman* [1943]), the recollections of the Confederate president’s wife, Varina Howell Davis (*Jefferson Davis* [1890]), and three letters written to London *Times* reporter Francis Lawley in 1897 and 1898. Unfortunately, Evans’s own contribution is slight. Therefore, the reader will leave this colorful, well-written, and easy-to-read narrative with a sense of frustration because so little has been added to what is already known. There are few new insights into Benjamin as senator, member of the cabinet, and almost daily confidant of the Confederate president.

Evans rationalizes the need for this biography on the grounds that “Meade and Butler were both Southern historians unfamiliar with American Jewish history” and that Meade, in particular, was unable to “convey the harshness of the anti-Semitism surrounding Benjamin” (p. xiv). The four lines of inquiry that the author indicates he will pursue are putting Benjamin in a Jewish context, discussing the multifaceted nature of the Benjamin-Davis relationship, inquiring into the evolving attitudes of both men toward slavery, and analyzing Benjamin’s personality—four altogether admirable goals. Not enough data exist, however, to support many of Evans’s conclusions, which, I suspect, reflect more the author’s own background growing up as a southern Jew in the 1940s and 1950s.

We know even before picking up this biography

that Benjamin was a target of anti-Semites during the Civil War. This has been admirably detailed in Bertram Korn’s *American Jewry and the Civil War* (1951) and Myron Berman’s *Richmond’s Jewry* (1979). But can we be sure that “Jews in the South were especially proud of Benjamin’s achievement, because it validated their legitimacy as Southerners” (p. xxi), that the “problems Benjamin experienced are not very different from those of other Jews in the South who have had to deal with having power, with race, with religious fundamentalism, with anti-Semitism” (p. xvii), or that being Jewish “was a burden, an inhibition to advancement, a restraint upon success” (p. 29)? I have no doubt that anti-Semitism existed in the nineteenth-century South (as it did in the North), that many Jews were victimized by it, and that Benjamin having been born a Jew provided opportunities for attacks from vitriolic bigots. But the faith of his ancestors did not prevent him from achieving what most observers would conclude was both financial and political success.

Evans would have been wiser to have written this biography as a historical novel. He would then have been able to blend his splendid writing skills, fertile imagination, and intimate knowledge of the southern Jewish experience. But as history and biography this book is less than satisfactory. There is simply an insufficient amount of information available to warrant the kinds of analyses that the author has made in his attempt to re-create the life of this very private person.

LEONARD DINNERSTEIN
University of Arizona

RICHARD NELSON CURRENT. *Those Terrible Carpetbaggers*. New York: Oxford University Press. 1988. Pp. x, 475. \$24.95.

This extended, revisionist treatment of the lives of ten of the most prominent carpetbaggers argues that, despite the Reconstruction era “Democrats’ propaganda technique of the Big Lie” (p. 424), which successfully blackguarded these men then and later “as villains of the most despicable sort” (p. 264), they never fit the stereotype of manipulative adventurers who despoiled a wounded land for their own profit. Richard Nelson Current’s ironically titled book harmonizes readily with the current assessment of the nature of Reconstruction.

Most of the people whose experiences appear here are familiar, from Adelbert Ames and Daniel Chamberlain to Albion Tourgée and Henry Clay Warmoth (as well as Powell Clayton, Albert Morgan, Harrison Reed, Robert Scott, George Spencer, and Willard Warner). Current tells the story

as they saw it and recorded it, relying effectively on the papers, diaries, and memoirs of each to detail aspects of the whole. He does not present full and separate biographical sketches. Rather, each person enters at a different moment, and his experience at a given point fleshes out part of the story. Current draws first from one, then another, to unfold a dark and bloody ground. Diverse in background, most originally soldiers, all Republican radicals, they entered the South as federal officeholders or aspiring entrepreneurs. Current's fully textured description suggests that their experience in the South was closer to their own perspective than to that of the hostile and unwilling white native population. Their lives proved to be anything but easy. They were harassed, ostracized, resisted, and, ultimately, violently defeated. Few prospered for long, many stayed on in the South, others were driven out.

Their involvement in politics was inevitable and their undoing. They entered the political universe not as a means to gain patronage and loot as much as to defeat the unremitting hostility of southern whites. As Mississippi's Ames remarked, "I do not think I am a politician by nature, but rather by necessity" (p. 306). But their commitments inflamed hostility toward them. The issue was not venality, corruption, or malignant purpose but always race. While they were neither revolutionary nor particularly adventurous in their objectives, they were committed to black political participation and civil rights in keeping with the Fourteenth and Fifteenth Amendments. The carpetbaggers caused "a disturbance in the relations between the races in the South. This was the basic reason for the anti-carpetbagger animus" (p. 425), then and later.

They failed as politicians. The difficulties of governing bankrupt states, amid a hostile white population increasingly violent in its resistance, were enormous and ultimately devastating. As one Ohio soldier wrote home in 1869, "The people are sullen and brutal, with little regard for human life, and none at all for law" (p. 173). As a result, the "pistol and the knife" became "more potent than the ballot" (p. 131). Current devastatingly demonstrates the limits of conventional politics in this era. As guerrilla warfare raged in the South, Republican administrations in Washington faltered. Political leaders on the scene proved unable to find a way through conflicting pressures, racism, the fear of public opinion, their own factional concerns, and the extraordinary intransigence of the defeated. In such circumstances, the carpetbaggers were helpless, if not hapless.

All of this is told sympathetically and with a wealth of detail. Current traces, with great effectiveness, the harsh situations these men experi-

enced. There are clear (and unavoidably repetitious) descriptions of the social ostracism, intimidation, ugliness, violence, and failure that was each carpetbagger's lot. He does not argue that his selected ten are rigorously representative of the whole group. But they were among the most prominent, and Current's unsurpassed ability to evoke the era certainly suggests that their stories illuminate the larger whole. He follows each of them into retirement and the long years many lived after Reconstruction ended. Many did well; others did not. Ironically, some lived long enough to clash with Dunning School graduate students righteously seeking to put the surviving carpetbaggers in their place through loaded interview questions reflecting the anti-radical attitudes and assumptions of the pre-1954 era.

As the last suggests, Current is also telling a moral tale. While never overtly partisan, he is sympathetic to the carpetbaggers' perspective: he sees the postwar South largely as they did. His quiet method is devastating to those who accepted and extended the untrue stereotype, from contemporary southern Democrats, who knew no better, to the later generations of professional historians, popularizers, and politicians. This is, in short, good, solid description of the best kind that tells a story well and makes a persuasive argument. If the details and conclusions no longer surprise us, the book rounds out our revisionist portrait of the era. It stands alone as a major statement on the subject and will materially assist others who will weave more elaborate tapestries of general understanding.

JOEL H. SILBEY
Cornell University

WILLIAM CLARK GRIGGS. *The Elusive Eden: Frank McMullan's Confederate Colony in Brazil*. Austin: University of Texas Press. 1987. Pp. xi, 218. Cloth \$25.00, paper \$9.95.

The decision made by several thousand inhabitants of the Confederate states to abandon home and country rather than be reconstructed in the wake of the American Civil War offers one of the more quixotic footnotes to American history. Exiles betook themselves to Canada, to Mexico, to Venezuela, and to Brazil. The participants in this exodus seem to have achieved relatively little. Their successes were few, their impact on the host societies was minimal, and most soon returned to the United States. Occasional scholarly notice of these exiles has done little more than sketch out the scope of the migration. A broad survey of their backgrounds, especially in contrast to those who chose to stay, of the conditions under which they

were wooed and welcomed into host societies, and of the reasons for their lack of success and their return home remains to be written. William Clark Griggs's book addresses few of these larger concerns.

In January 1867 a group of about one hundred Texans set sail from Galveston, bound for Brazil under the leadership of Frank McMullan. After a series of misadventures, including shipwreck on the Cuban coast and a miserable month in New York, the adventurers arrived at lands in southern São Paulo, which the imperial government had surveyed for McMullan and his partner William Bowen. The McMullan-Bowen colony was one of five government-sponsored *colônias* established on public lands by American exiles between 1866 and 1868. None of them lasted. Isolated, unfamiliar with the methods of cultivation and the crops appropriate to these climes, miles from markets or roads by which to export their produce, in a land of strange tongue and custom, the immigrants soon decamped. Those who did not return to the United States gravitated to the area west of Campinas, São Paulo, where Colonel William Norris and several other families had purchased privately held lands on old coffee plantations. Vila Americana survived, served by the Paulista Railroad after 1871, and retained its character as a southern outpost well into the twentieth century.

Despite Griggs's claim that the McMullan expedition "would result in changes of incalculable value to Brazil" (p. 4), the story he purveys falls far short of the promise. It is limited by a focus on individuals and the narrow concerns of the McMullan-Bowen settlement. McMullan, who died of tuberculosis within months of the settlers' arrival in Brazil, is sketched as a shadowy figure, more interested in tales of gold in the *sertão* than in the settlement. His partner Bowen gets even shorter shrift. Although the author has dug diligently in archives and private papers, searched out the descendants of some of the settlers, and provided the appropriate scholarly apparatus, the story remains episodic and choppy. For those interested in displaced Confederates, this volume has limited value; for the serious student of Brazilian or Texan history, it is next to useless.

GEORGE P. BROWNE
Seton Hall University

LARRY SCHWEIKART. *Banking in the American South from the Age of Jackson to Reconstruction*. Baton Rouge: Louisiana State University Press. 1987. Pp. xiv, 367. \$35.00.

Caveat emptor. This book is a revision of Larry Schweikart's 1983 doctoral dissertation. He has

chosen an important subject and produced an expensive, poorly written book that has little to recommend it except misguided eccentricity.

In fact, there are two books within these covers. While they are related, one is a rather ordinary and somewhat unbalanced monograph on the evolution of southern banking law from "the Age of Jackson to Reconstruction." The majority of the book is devoted to the 1830s. The author does not seem to care about the context in which these laws were passed or southern politics in general.

This portion of the book is rooted in Schweikart's useful collection of statistics on southern banks. He uses them to argue that the banks in the Old South, open to competition, were stronger than those in the New South, dominated by regulation. He explains the differences in banking policy in the two subregions in terms of "the predominance of commerce in the states of the Old South, which contrasted with the agriculture-based economies of the New South" (p. 143). Schweikart also insists that the underdevelopment of the antebellum South was not the result of a want of credit; in the decade before Fort Sumter, there was a massive expansion of the number of banks in the southern states.

His arguments are rather unconvincing. If Schweikart deals with his primary sources as loosely as he handles his secondary sources, one cannot know what to believe. Although there are numerous examples in which the footnotes have no relation to the text, Schweikart's discussion (pp. 60–61) of Peter Temin's revisionist view of the causes of the Panic of 1837 can serve as an illustration of some of the problems. It involves three paragraphs and three footnotes. Schweikart's version of the traditional view, which he terms the "Soundness School," is a caricature rooted in a basic error in fact (Jackson did not remove the deposits in 1832) and a completely implausible scenario.

The footnotes to these paragraphs are odd. The first paragraph refers to Temin but does not cite his book. The second paragraph begins with a reference to Bray Hammond. The footnote is to a single 1833 letter. Finally, the third paragraph is explicitly on Temin's thesis. The footnote refers to his book but not to the correct pages.

However, such minor matters could not bother Schweikart, for he has bigger fish to fry. It is true that the debate on antebellum banking has been among liberals. Schweikart has drawn the subject into the sectarian squabbling of the New Right. The basic thesis of this book is that the Jacksonians were unwittingly centralists. In the long run, they were no different from the Whigs or the Republicans. His first footnote informs the reader that "most Democrats *thought* they opposed a growing

centralized government although they did not actually do so" (p. 1). No one in the nineteenth century seems to have been interested in free enterprise. By page 9, it is clear why: they had not read Leo Strauss.

Schweikart has followed the leader to find the "real" Jacksonian policy that had eluded not only historians but most Jacksonians. Appropriately, his case hinges on a single letter by Amos Kendall. It is also the only Kendall letter he has read. Using this, Schweikart insists that the Jacksonians had a "five point plan" to gain central control of the money supply beginning with the destruction of the Bank of the United States. That is not what most Jacksonians were saying, including the somewhat befuddled "Old Hero," the party press, and candidates on the stump. It is also not what happened. By 1860, there were thousands of different currencies circulating in the country. Schweikart thoroughly confuses the reader by not making clear the differences between state-chartered banks, state-owned banks (usually mixed corporations), free banks (of various types), and private banks.

Schweikart essentially argues that, on an issue where most historians have found party conflict, the parties were remarkably similar. The Left has constantly said such partisan quibbles were unimportant. We now have a voice from the Right, eager to confront Marxists at every turn, accusing nineteenth-century southerners of not trusting the marketplace.

WILLIAM G. SHADE
Lehigh University

GAIL WILLIAMS O'BRIEN. *The Legal Fraternity and the Making of a New South Community, 1848-1882*. Athens: University of Georgia Press. 1986. Pp. 231. \$23.50.

Gail Williams O'Brien's book successfully applies the techniques of a local study, investigating social, cultural, economic, and political aspects of public and private lives, to a nineteenth-century southern "community." O'Brien's work challenges the most recent "standard" theme that industrialization in the post-Civil War South occurred because the planter aristocracy had been removed or weakened and replaced by an aggressive, entrepreneurial middle class. O'Brien argues that in Guilford County, North Carolina, an entrepreneurial group—the legal profession—dominated public life and provided economic leadership on a continuum from 1848 to 1882.

The legal profession, although small in number, played a crucial unifying role in economic devel-

opment and political actions. Lawyers were the glue of society—the elite professional group that was not divided by agrarian-commercial interests, rural-urban conflict, or partisan politics. O'Brien uses Clifford Geertz's thick description technique, in part because of necessity (only a total of twenty "high powerholding attorneys" are listed for the four census cohorts of 1850-80) but more so because of the author's integrated research approach, combining social science methodology and traditional historical techniques.

O'Brien concentrates on federal census data to quantify the community's power structure and "focuses on opportunities for wielding power or influence in the public arena" (p. 20). This study predictably demonstrates that political institutions offered the main chances to influence people and events. Lawyers were able to play a critical role in public life, because partisan politics did not divide them professionally. For example, O'Brien notes that three post-Civil War law partnerships in Guilford each had one Democrat and one Republican. Lawyers thus served the community as a conservative, compromise-inducing core, even in the postwar period.

More important, the lawyers of Guilford were able to bridge the agricultural and business communities by their professional activities, the geographic location of their residences, their land-ownership patterns, and their entrepreneurial actions. The "capitalistic consensus" of the profession remained firm throughout the period—the goal was "making money" (p. 79). They were indeed "planters-businessmen-attorneys" (p. 79), but, despite their conservative social role, they were economically innovative for the North Carolina Piedmont.

The author's efforts to integrate a study emphasizing the crucial role of the legal profession within the broader development of an industrial society might have benefited from more open and thorough coordination with major studies of the nineteenth-century American legal profession that have been done in the past decade and that employed many of the same social science methodologies.

This community study suffers from the usual difficulties in linking the local subject to the larger world—in this case, the "New South" and, beyond that, a burgeoning, cosmopolitan, heterogeneous, industrial American society. Despite the excellent quality of this study of the evolution of a nascent industrial society in a rural, small-town, agrarian, stable region, confirmation of a new reinterpretation of the rise of the "New South" and the legal profession's crucial role will await similar studies

of urban-industrial regions of the "old" and "new" South.

GERARD W. GAWALT
Library of Congress

ROBERT C. KENZER. *Kinship and Neighborhood in a Southern Community: Orange County, North Carolina, 1849–1881*. Knoxville: University of Tennessee Press, 1987. Pp. xiv, 251. \$28.95.

The author draws a provocative picture of one North Carolina county over an important forty-year period. Orange County is probably best known for its towns: Hillsborough, its charming county seat; and Chapel Hill, long home to the University of North Carolina. But it is the rural neighborhoods and their nature and composition on which Robert C. Kenzer concentrates.

Other scholars exploring southern communities have tended to emphasize the importance of family ties and localism but none so much as Kenzer. He depicts remarkable stability and continuity that the Civil War only shifted rather than destroyed. Indeed, neighborhoods in this account were largely collections of people with numerous ties of blood and marriage. He argues that most people attended school and church, made purchases at the general store, even chose marriage partners within their neighborhoods, and knew little else. Given such strong bonds to kin and place, class divisions held relatively little meaning for them. Kenzer documents how sons of nonslaveholders married the daughters of slaveholders and landholding men married women from landless families. To be sure, the Civil War slightly altered this situation as it lowered the social status of landless whites. Moreover, in these neighborhoods, rates of persistence were generally high, especially among the long-settled families, which had both successful and unsuccessful branches. While the Civil War briefly pulled young men away from their communities, by the 1870s persistence rates for young white men had actually increased over the 1850s. Even the rapid growth of Durham as a tobacco manufacturing town did not cause rural Orange County residents abruptly to change their habits—many who moved to Durham in the 1870s had been living in the nearby townships.

All students of southern social history will be intrigued by Kenzer's study. He traced a population rather than a sample through four manuscript censuses—truly a heroic task—and also extensively used county records and private papers. This microscopic focus does convincingly detail the clusters of extended kin found in neighborhoods. But some will question his interpretations of his findings. For example, he shows that, at

most, approximately one-half of men in their twenties remained in Orange County from one decade to the next. While Kenzer regards this as relative stability, these statistics also indicate considerable out-migration and suggest the existence of a pool of transients, possibly landless laborers. Similarly, while Kenzer attributes the higher persistence rate for young white men in the 1870s to greater opportunities at home (their inheritances were larger because their brothers had died in the war), this might well be viewed as an apathetic reaction to greatly reduced chances of success anywhere. But historians, regardless of the extent to which they agree with Kenzer's interpretation, will find that his book provides a new and much needed focus on important parts of southern life.

JANE TURNER CENSER
Frederick Law Olmsted Papers
American University

ANN LEIGHTON. *American Gardens of the Nineteenth Century: "For Comfort and Affluence."* Amherst: University of Massachusetts Press, 1987. Pp. 395. Cloth \$35.00, paper \$14.95.

Readers familiar with Ann Leighton's *Early American Gardens* (1970) and *American Gardens in the Eighteenth Century* (1976) will find this third book in her series, published posthumously, similarly organized and written. Like the earlier volumes, it is not really so much a history as a series of impressionistic glimpses into an open-ended variety of topics. The author concentrated on the years between 1800 and 1900, years that fail to mark either the beginning or the end of significant change in history or art. Moreover, the book's organization into twenty loosely arranged, somewhat uneven, and shortish chapters, which do not always follow a perceptible sequence, prevents clear historical images of gardens in, say, 1820 as opposed to 1890 or, for example, in Massachusetts, as opposed to Louisiana.

Leighton was indifferent, if not averse, to tracing aesthetic change in gardens, and the impression left is often blurred. Because she was a landscape architect as well as a consultant, her omissions are somewhat surprising. I perceive that Picturesque tenets dominated American landscape design in the nineteenth century until the 1840s when A. J. Downing challenged this aesthetic with a renewed effort to attain "the Beautiful." Leighton illustrates Downing's plan showing the Picturesque (p. 169) but inexplicably omits his effort to extol the Beautiful. I also maintain that the reaction against the Picturesque was much less intense in America than it was in England. But the reaction of William Robinson and Gertrude Jekyll

there between 1880 and 1900 to the anti-Picturesque mid-century gardens identified with Joseph Paxton was deeply felt in the United States, a fact almost denied by Leighton.

The diversity of subject matter is striking. Chapters are devoted to, among other topics, horticulture, American scenery, Central Park, women, and societies and shows. Thus, the book is about more than gardens. A foreword and afterword focus on the summerhouse "as a symbol of American nineteenth-century gardening" and depict Americans as "nationally blessed 'with comfort and affluence'" (p. 297). But the book does not open auspiciously. In the first paragraph (p. 3), Leighton writes that Michel Guillaume Jean de Crèvecoeur "wrote enthusiastically of resting" in William Byrd's summerhouse in the "early eighteenth century." But Crèvecoeur was not born until 1735. And Leighton, in an opening statement on the summerhouse as symbol, could have used a more effective and timely experience.

The second section is the most historical. Leighton includes a chronological sequence of chapters, beginning with Dr. David Hosack, New York City botanist, and ending in the 1890s with Frederick Law Olmsted at Biltmore in North Carolina and Charles Eliot and the founding of the Arnold Arboretum in Boston. Surprisingly, we are not told of Eliot's and Olmsted's role in forming the first American collegiate program in landscape architecture at Harvard University or of the concurrent founding of the American Institute of Landscape Architects in 1899. Either subject would have made a fitting conclusion. Moreover, none of the six chapters in this section focuses on Downing or Olmsted, the outstanding landscape architects of the century.

Chapters in the third section are also diffuse. To find the influence of Japanese (and Chinese) gardens, as well as Olmsted's work on Biltmore, contained in a chapter entitled "Specialty Gardens" is disarming. The late nineteenth-century influence of Japanese gardens on the West, like that of Japanese architecture and prints, was certainly profound enough to be considered more than a "specialty." Leighton included a seventy-nine-page appendix of plants most commonly used in nineteenth-century American gardens, but the books listed as sources (p. 379) all date between 1839 and 1881.

Sources are not given for many of the prints and drawings—unnumbered—that illustrate the text, which is also the case for the numerous and often lengthy quotations. Leighton obviously had an aversion to the standard footnote that cites source and page; not one such note can be found, and the book's value to scholars is thus impaired. One chapter consists of seven pages almost entirely

devoted to unanalyzed quotations from Frances Trollope's 1832 book, *Domestic Manners of the Americans*. Despite a number of eccentricities, some of which might have been alleviated by more rigorous editing, Leighton's serious interest and experience in gardening has produced an informative and useful book on a subject that much needs study but that still awaits an exhaustive work.

JAMES D. KORNWOLF

College of William and Mary

KATHRYN GROVER, editor. *Dining in America, 1850–1900*. Amherst: University of Massachusetts Press and the Margaret Woodbury Strong Museum, Rochester, N.Y. 1987. Pp. ix, 217. Cloth \$25.00, paper \$12.95.

This pleasant volume, gloriously illustrated and imaginatively conceived, is at once more and less than its title implies. Its six essays were first presented as symposium papers to complement an exhibition on the material culture of American dining at the Margaret Woodbury Strong Museum, Rochester, New York. Their collective purpose, as assembled here, is to appraise the rituals, social behavior, diet, accouterments, and architecture of the middle-class Victorian dining room. Although largely succeeding in that goal, the essays nonetheless fail to satisfy some natural expectations.

Each essay is interesting in its own way, but only three deal directly with the stated topic. W. J. Rorabaugh's study of middle-class drinking habits tells us more about the temperance crusade and class divisions than it does about dining. David W. Miller's exploration of the technological "revolution" in food preparation between 1830 and 1920 is more concerned with the kitchen than with the dining room. Eleanor T. Fordyce also focuses on the kitchen with an essay titled "Cookbooks of the 1800s." Fordyce contends, quite rightly, that cookbooks can tell us much about changes in middle-class table manners, etiquette, and family life, but, rather than discuss these changes, she focuses on the evolving purpose of the cookbook and the origins of the cooking school movement. More than half of her essay, incidentally, deals with the period before 1850. More to the point are the essays by John F. Kasson on dining-room etiquette, Clifford E. Clark, Jr., on the social importance of the dining room, and Dorothy Rainwater on dining-room silverware.

Whatever their particular subjects, the contributors seek unity through several broad themes. The most important of these themes is the growing self-awareness and self-confidence of America's middle classes during the second half of the

nineteenth century. Many middle-class cultural concerns fostered by this growth—including personal propriety, social respectability, conspicuous consumption, and emulation of the wealthy—are illustrated and explored. Lesser themes revolve around the growing complexity of middle-class life, the middle-class preoccupation with style and ornamentation, and the expanding social and domestic roles of middle-class women.

Still, some subjects one might expect to find in a book on dining are slighted or missing altogether. For instance, nothing is said about dining out. Although looking outside the confines of the family dining room may not have conformed to the book's stated purpose, dining out seems to be at least as legitimate a topic as the temperance crusade and cooking schools. Dining out became quite fashionable during the second half of the nineteenth century, and its role in defining middle-class self-awareness would have fit nicely into the book's broader themes. Similarly, an essay on domestic servants, specifically cooks, waiters, and waitresses, would have contributed significantly to the book's adopted themes. A discussion of diet—what middle-class Americans ate—is lacking, too, as is any mention of diet-related health problems. Perhaps the most glaring deficiency, however, is the book's geographic myopia. As with so many other discussions of "United States" history, the country seems to exist largely east of the Mississippi River and north of the Ohio River. The South is not mentioned at all. The Far West is important only as a source of silver for New England tea sets. Only Minnesota (barely west of the Mississippi) receives any prominent mention (in Clark's essay). Discussion of the provinces would have been useful, if only to assure us that everyone else was aware of and conforming to trends in the Northeast.

In short, the book is a useful and enjoyable introduction to Victorian dining, but it is not all it could have been or purports to be.

DANIEL E. SUTHERLAND
University of Arkansas

KEVIN J. CHRISTIANO, *Religious Diversity and Social Change: American Cities, 1890–1906*. New York: Cambridge University Press, 1987. Pp. xvii, 239. \$34.50.

Although historians have analyzed religious developments in the late nineteenth and early twentieth centuries, few of them have utilized the available city-level federal census data on religion. Kevin J. Christiano uses those data from 1890 and 1906 to analyze religious diversity in the United States and demonstrate the value of broadening our study of

religion to include ecological information on church membership.

Since the original manuscript census returns on religion for 1890 and 1906 have been destroyed, Christiano relies on published data for the 122 cities with a population of at least 25,000 in 1890. Analyzing the data for those two years cross-sectionally as well as longitudinally and employing multiple regression analyses, he provides an in-depth, statistical analysis of religious diversity in America. His primary purpose is to determine how socioeconomic and demographic factors such as urbanization, immigration, industrialization, literacy, and the presence of alien subgroups within a city can account for either the religious diversity among Protestants, Catholics, and Jews or the religious diversity among Protestant denominations.

The book is a careful and thoughtful analysis of religious diversity based on city-level census data. Christiano is well read in the sociological and historical literature on religion and places his findings within the broader context of the secondary literature. He concludes that population growth and industrialization by themselves had a limited influence on religion in American cities at the turn of the century but that the presence of cultural and ethnic subpopulations within those cities had a significant impact on shaping the contours of religious diversity. Religious diversity also contributed to a growing secularization as well as to a consolidation of some of the major conflicting religious groups.

Christiano is well aware of the potential limitations of the church membership data but does not believe that this seriously affects his analyses. Nevertheless, further inquiry into the quality of those data is warranted. Indeed, he acknowledges that Gregory Singleton, using the same data for Los Angeles but supplementing them with information from parish and denominational records, arrived at quite a different index of religious diversity.

The entire analysis also appears to be limited by the small number of cases available. In his various chapters, Christiano makes use of different sets of independent variables rather than including all of them in one equation—presumably because of the small sample size. Consequently, he is unable to control simultaneously for all of the relevant factors. While he attempts to treat this problem in an extended methodological footnote, the discussion is not entirely reassuring—particularly because there is such a high correlation between some of his variables (the proportion of nonwhite and the proportion of foreign-descent are correlated in 1890 at $-.716$). As a result, one often wonders exactly what is being measured by some of his

independent variables, since the sample size is small and some of the most important indicators are highly correlated with each other (unfortunately, he does not provide us with a correlation matrix of all of the variables used in his regressions).

Although the book is generally clearly written, historians uninitiated in statistical analysis may find it difficult, but not impossible, to follow, since the short chapters are rather densely packed with regression results. Nevertheless, the book is well worth reading because it opens up new areas for scholars of religion to explore and challenges us to utilize hitherto neglected religious census data.

MARIS A. VINOVSIS
University of Michigan

PETER J. LEWY. *To the Columbia Gateway: The Oregon Railway and the Northern Pacific, 1879-1884*. Pullman: Washington State University Press. 1987. Pp. xxv, 202.

In the three decades following the end of the Civil War, the vast area of the Pacific Northwest dominated by the Columbia River and its tributaries was opened for economic development. Some visionaries promoted the concept of the "Columbia Gateway" that urged the creation of a transportation network in the region to link the markets of the Midwest and the East to the Orient. This volume focuses on that concept and how, during the period from 1879 to 1884, railroad construction in the Columbia watershed turned part of that vision into reality. A British-born mining engineer living in British Columbia, Peter J. Lewy describes the origins of the Northern Pacific Railroad, the creation of the Oregon Railway and Navigation Company, and the rise and fall of Henry Villard's regional transportation empire.

The volume is a highly detailed account of the development and construction of rail routes along the Columbia and Snake rivers and across the "Interior Plains" of the Pacific Northwest. Lewy presents the story of river transportation on the Columbia, then chronicles the formation of railroads in the region. Transportation networks reflected the desire of residents to expand the production of wheat and the sale of timber and other natural resources as well as the intense competition between the Northern Pacific and the Union Pacific railroads to obtain dominance in the Pacific Northwest. Locally promoted carriers often became pawns in the larger conflicts involving Jay Cooke, Henry Villard, Jay Gould, C. Huntington, and other leaders in the industry. The dominating figure in the book is Villard, and the decisions he

made often directly influenced the fate of town promoters, land speculators, miners, and farmers struggling to develop the area.

Based almost entirely on secondary accounts, local newspapers, and published memoirs, the volume is cluttered with details on steamboats, bridges, railway construction, wrecks, and daily operations. Because corporate records at the James J. Hill Reference Library, the Minnesota Historical Society, and the Union Pacific headquarters were not consulted, the analysis does not move far beyond James Blaine Hedge's study of Villard published more than fifty years ago. It is also disconcerting to find in a university press book the statement that the Civil War began in April of 1862 (p. 1) and that Pullman car porters were "mahogany hued" (p. 103).

The book does demonstrate the rapidity of economic growth in the Columbia Basin as the doubling of railroad mileage in less than six years led to substantial increases in agricultural and mineral production. Railway development also served to stimulate urban growth, particularly in communities with railroad facilities. The equally significant story of the decline of local control receives less emphasis. The book offers little that is new and will find a limited audience.

KEITH L. BRYANT, JR.
University of Akron

SCOTT L. BOTTLES. *Los Angeles and the Automobile: The Making of the Modern City*. Berkeley and Los Angeles: University of California Press. 1987. Pp. xiii, 302. \$25.00.

From the turn of the century to the present, Southern California has led the nation and the world in automobile registrations per capita and early gained the reputation among automobile manufacturers as "a bottomless pit" for new car sales. A lifestyle based on mass personal automobile first developed in Southern California in the 1920s. The region is, therefore, of considerable interest to anyone wishing to understand the automobile revolution.

The title of Scott L. Bottles's book is misleading, for it is not a comprehensive study of the impact of the automobile on Los Angeles but rather a much narrower study of the public debate over transportation policy in Los Angeles up to 1947. Bottles explains that a great public reaction against the traction companies occurred during the Progressive era, when "railway executives constantly found themselves surrounded by angry patrons demanding improved services and increased government control over public transit" (p. 23) and that "commuters and shoppers in Los Angeles

began using their automobiles [after 1914] because fifteen years of inadequate streetcar and interurban service finally convinced many citizens to seek alternative means of transportation" (p. 56). He follows closely the debates in the local newspapers and the Los Angeles City Council over the traffic congestion and decentralization caused by mass motorization and the attempts by downtown business interests to keep alive a viable mass transit rail system. Survival of the transit system proved impossible because of the inability of the traction companies to finance improvements and the unwillingness of voters to subsidize them. The most extensive treatment yet is given to such controversies as the 1919 parking ban, the 1924 Major Traffic Street Plan, and the 1925–26 proposal to build the Union Station. "The parking ban and the Major Traffic Street Plan brought together disparate groups of people seeking to secure the automobile's place within the Los Angeles transportation system" (p. 125), he concludes. "With such a consensus, the electorate willingly taxed and assessed itself to build a new network of streets." Voter approval of Union Station marked defeat of the railroads' counter-proposal to build elevated tracks for their trains to relieve downtown traffic congestion. The conclusion of civic leaders came to be "that the ultimate solution to the traffic problem lay in decentralization" (p. 159). State legislation for highway construction passed in 1947; the Collier-Burns Act "culminated the region's quest to solve its transportation problems by ensuring the construction of an urban freeway system" and "succeeded in putting forth the freeway network as Los Angeles' answer to twentieth-century urban ills" (p. 233). An epilogue summarizes developments since then and concludes that "no matter how much social critics and urban planners push for rapid-rail systems, it is unlikely that urban residents will give up the freedom and convenience afforded by the automobile" (p. 254).

Bottles unfortunately aims his overall argument against Bradford C. Snell's 1973 allegation that the decline of mass transit in Los Angeles and other American cities was the result of a conspiracy by General Motors and other automotive interests to buy up and then dismantle and destroy healthy urban rail transit systems. Here he beats a dead horse, for the Snell allegation and its supporting evidence have been widely discredited, most persuasively by General Motors itself in its rejoinder to Snell's charges. Bottles also sets up a straw man in his claim that Snell "implies that the rise in popularity of the automobile in Los Angeles occurred in large part because of a General Motors conspiracy" (p. 3). Nowhere does Snell in fact imply this absurdity. Moreover, a much more

thorough critique of Snell's allegation and the role of automotive interests in the mass motorization of cities can be found in David J. St. Clair's *The Motorization of American Cities* (1986).

Los Angeles and the Automobile offers a wealth of detail on the public debate over transportation policy in Los Angeles up to passage of the 1947 Collier-Burns Act. This is a laudable contribution. The book delivers, however, much less than its title promises. The reader seeking a comprehensive understanding of the role of mass motorization in the making of modern Los Angeles is still much better informed by Ashleigh E. Brilliant's "Social Effects of the Automobile in Southern California in the 1920s" (Ph.D. dissertation, University of California, Berkeley, 1964) and Mark S. Foster's "The Decentralization of Los Angeles during the 1920s" (Ph.D. dissertation, University of Southern California, 1971) and by Martin Wachs's excellent survey, "Autos, Transit, and the Sprawl of Los Angeles" (*American Planning Association Journal* [1984]). The reader already familiar with the literature on the Southern California automobile culture will find a mass of new detail documenting familiar generalizations.

JAMES J. FLINK
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Irvine*

ANTHONY M. ORUM. *Power, Money, and the People: The Making of Modern Austin*. Austin: Texas Monthly, 1987. Pp. xiv, 404. \$16.95.

This is a chatty history of why and how the city of Austin, Texas, expanded in the twentieth century. Anthony M. Orum maintains that it was generally a result of a strong vision of the future and hard work on the part of Austin's early leaders. The money and power that flowed from the federal government in the 1930s played a crucial role, too. Considerable attention is given to such historical developments as the establishment of the Lower Colorado River Authority (LCRA), the evolution of the black community, tribulations at the University of Texas, and continuing clashes "between capitalism and democracy" (p. xiii). The driving forces behind Austin's expansion changed in the late 1960s as the urban entrepreneurs were replaced by market entrepreneurs, who cared less for the unique place that was Austin and more for private fortunes. Ultimately, the city's springs and lakes and forested hill country beauty were compromised by real estate brokers and the burgeoning population.

Two or three city and area maps and a bibliography would have been useful. A more serious shortcoming is that a number of existing second-

ary sources on blacks, the University of Texas, politics, and politicians in Texas, as well as two articles by David Olson specifically on Austin, were apparently not consulted; Orum often pieces together histories of events with only a few interviews or letters. It causes him to miss some of the context and a few pertinent details in some of the events he describes. Even the accuracy of some details may be questionable.

In the chapter on "Taming the River," for instance, Orum cites one veteran politico as his source for the claim that the Dallas delegation in the Texas House agreed to support the creation of the LCRA in exchange for the Texas Centennial being placed in Dallas. But on the final passage of the LCRA bill in 1934, the *House Journal* (pp. 328–29) shows Dallas representatives voting two yes, two no, and two absent. Orum does not use the *Journal*. The author wonders how the Brown and Root Company received a second five million dollars for the low dam at Marshall Ford. Robert Caro's *Path to Power*, also uncited, has several pages on it. Other sources that might have proved useful for this chapter include the Sarah Hughes Papers, the Sarah Hughes chapter in the Ann Crawford and Crystal Ragsdale book, *Women in Texas*, and Harold Ickes's note on the LCRA in *The First Thousand Days*.

The book is more episodic and impressionistic and less comprehensive than most urban histories. It is generally well written and is the result of prodigious work, especially the recording of some 180 interviews. Moreover, Orum's history of urban and market entrepreneurs seems to justify his pessimistic conclusion that "private property always reigns victorious over the common good" (p. 308), at least in the case of Austin. *Power, Money, and the People* is of dubious value as an intended case study, but it ranks among the better and more interesting biographies of southwestern cities.

GEORGE N. GREEN
University of Texas,
Arlington

MAUREEN A. FLANAGAN. *Charter Reform in Chicago*. Carbondale: Southern Illinois University Press. 1987. Pp. xii, 207. \$23.95.

Maureen A. Flanagan's book is a study of the failure in 1907 to secure a new municipal charter for Chicago. Sympathetic to the forces that defeated the charter, Flanagan divides its proponents and opponents into capital versus labor, the rich versus the poor, and white Anglo-Saxon Protestants versus ethnic minorities. Supporters of orderly, economical, and business-oriented gov-

ernment favored the charter; advocates of popular rule, municipal ownership, progressive taxation, and social welfare were in opposition. Hence, the charter was a microcosm of what Flanagan calls the political culture of the city. The formulators of the charter were predominantly members of the business elite or its representatives. The charter convention, heavily weighted in favor of this enclave, excluded demands for municipal ownership of utilities, female suffrage, a more democratic public school system, heavier taxes for big business, keeping saloons open on Sunday, and other measures supported by the urban majority. Consequently, labor unions and other liberal organizations opposed the charter. Passage was further complicated by the state legislature because the rural majority in Springfield wanted to maintain control over Chicago. Supporters of the charter were able to accommodate modifications demanded by the legislature, but these compromises made the document more unpalatable for the opposition. A combination of these internal conflicts and external constraints resulted in the charter being overwhelmingly rejected by Chicago voters. This defeat prevented twentieth-century Chicago from having a centralized, adequately funded, and effective government and, according to Flanagan, was the main cause of the rise of the Democratic machine. The Democrats supplied the public services and government organization that a good municipal constitution would have provided.

Flanagan's book is both brief and repetitive. The two main arguments make frequent appearances. Opponents of the charter were no less reformist and no less broad-minded in their vision of the city than were the proponents, or, conversely, both sides equally represented special interests. The author thinks that current historians believe that business-oriented reformers in the Progressive period were not conservative defenders of their class and occupational interests and that workers and immigrants and their children were opposed to reform. In defense of this claim she cites Richard Hofstadter's *The Age of Reform* (1955) and some doctoral dissertations from the 1970s and articles in relatively minor journals. Flanagan should be more aware of the historiography of Progressive reform, which, since the 1960s, contains numerous findings regarding the conservatism and special-interest pleading of business leaders—reformers and the progressive impulses of labor leaders and ethnic politicians. Flanagan's other point that labor and political leaders were no less cosmopolitan in their urban outlook than were business leaders is more novel. But this contribution is insufficient compensation for a general lack of originality and thoroughness.

The lack of thoroughness is reflected in the unproven declaration that the urban machine was made possible by the absence of an effective municipal charter. Flanagan should have compared Chicago politics with other urban machines. Did Kansas City, which also had a long-lived machine, lack an effective city charter? She should be a little more careful, in this era of collapsing city services and the turn to privatization as a remedy, in giving unstinting praise to municipal ownership. Finally, since the United Societies for Local Self-Government played a pivotal role in the defeat of the charter, it would have been useful to have a membership profile of this organization in order to ascertain whether it was a front for liquor interests or an authentic representative of what Flanagan claims is positive and popular political reform.

FREDERIC COPLE JAHER
University of Illinois

DAVID D. VAN TASSEL and JOHN J. GRABOWSKI, editors. *The Encyclopedia of Cleveland History*. Bloomington: Indiana University Press, in association with Case Western Reserve University, Cleveland, Ohio. 1987. Pp. lv, 1127. \$35.00.

This encyclopedia is a monumental book in every sense, containing as it does 1,127 pages of small print. The end material alone consists of a ten-page subject guide and a thirty-three-page index, in even smaller print. The book must total over 1.5 million words with its short history of Cleveland and over twenty-five hundred entries on every conceivable topic concerning the city. There are illustrations as well: multiple maps of Cleveland's wards from 1840 to 1986, of civil subdivisions of Cuyahoga County from 1843 to 1986, of population from 1910 to 1980, and of ground transportation from 1860 to 1986, as well as single maps of other subjects. The editors, David D. van Tassel and John J. Grabowski, include tables showing all of the names of Cleveland's mayors, the names and locations of selected dance halls and ballrooms in Cuyahoga County from 1840 to 1980 (although the table of contents lists only the years from 1970 to 1980), the foreign-born population in the region from 1870 to 1980, immigration to Cleveland by country of origin from 1874 to 1907, outdoor statuary and monuments in Cleveland, radio stations in Cuyahoga County, and land-use controls in greater Cleveland.

The project took six years to complete, requiring the efforts of two hundred fifty contributors plus the financial support of foundations (including the National Endowment for the Humanities), companies, private organizations, and

individuals. In this respect, the encyclopedia resembles nineteenth-century county and city histories, except that it is a scholarly rather than a popular work. Given all of the effort, the book, at the price of thirty-five dollars, surely must be the greatest bargain since the twenty-five cent paperback.

There are several fundamental questions that must be asked when contemplating such an effort. The first question involves format. Why choose an encyclopedia instead of a multivolume history of the city? The editors do not explain their choice of this particular form. One must suppose that their selection was prompted by an assumption that an encyclopedia was more easily created than a detailed history that would entail more stylistic and narrative attention. Choosing this format does sacrifice elements of both style and narrative as well as create, necessarily, more repetition. The second question is related. Given the resources invested, was the product worth the cost? The answer is an unqualified yes. The encyclopedia is a success, combining scholarly elements with popular appeal, although the intended audience is somewhat unclear.

The reader's guide at the front of the book offers few clues to what the editors intended. From other internal evidence, the book apparently was designed as a civic monument to help refurbish the image of a city hit hard by economic recession. Obviously, too, it was intended as a necessary reference work for the libraries of schools and colleges in the region. Finally, it was meant to be a scholar's aid. Although it seems unlikely that the book was put together to attract a wider audience, the numerous printings after its publication suggest that this has occurred.

Because an encyclopedia depends, more than other edited works, on the editors' judgment for its organization, criticism of that organization may not be warranted, reflecting as it does a different perspective. In general, few such criticisms occur to me. The volume is divided into sections alphabetically with two kinds of entries: general ones of around four hundred words that constitute the majority and interpretive ones that are much longer and were written by specialists on the topics. Examples of the general entries are essays on the epizootic epidemic and the Vojan Singing Society; examples of the interpretive articles are ones on education and dentistry. Some of the entries, particularly the longer ones, have references to books or archival collections or both, which add to their usefulness for scholars.

The editors' criterion for inclusion was that "subjects, organizations, and events" must have been "central to the region's development." To a novice in Cleveland history, this requirement ap-

pears to have been met, although one wonders if some lesser topics did not creep in. The size of the volume is intimidating; perhaps less would have been more. Another decision the editors had to make was how to treat entries that were similar. Again, their decisions seem to make sense. A railroad buff might quarrel with the choice of combining entries of the Erie and Pennsylvania railroads into one on Conrail, but nonspecialists probably would not.

These are minor caveats, however. This pioneering work is a great success, and already other cities such as New York and Indianapolis have embarked on similar projects. Perhaps we will see the day when an encyclopedia of history may become as necessary to metropolitan status as a domed stadium or a professional football team.

DWIGHT W. HOOVER
Ball State University

MARGARET RIPLEY WOLFE. *Kingsport, Tennessee: A Planned American City*. Lexington: University Press of Kentucky. 1987. Pp. xii, 259. \$24.00.

Margaret Ripley Wolfe wrote this book in order to provide an account of "the first thoroughly diversified, professionally planned, and privately financed city in twentieth-century America" (p. 1). To accomplish this task, she consulted a wide range of secondary works and delved deeply into an impressive number of archives. The result is a piece of analytical scholarship that is carefully crafted and a pleasure to read. The topics covered are the cultural and intellectual foundations of Kingsport, the physical design of the city, the industrial base, associational activities, the crisis of depression and war, the decline of community cohesion, and the ultimate loss of civic vision. Wolfe is at her best when exploring the complex dynamics that led from an enthusiastic (almost manic) founding to the unraveling of a model city. Local history buffs who insist on a simple story with heroes and villains will have to look elsewhere. Wolfe has a keen sense of irony as she explores the tangled twists in the history of a community. The various characters, even Brahmins such as John B. Dennis, are portrayed as people who are consciously part of a network of social, economic, and political interactions. It is these interactions that are the focus of Wolfe's analysis. Surely, this is the way to study a community.

There are, alas, two major problems. The chapter on cultural and intellectual foundations is at once too broad and too narrow. It is too broad because Wolfe introduces a handful of general interpretations of the American experience in

order to support her assertion that "Kingsport's underpinnings were based on a potpourri of American ideology" (p. 11). After a brief discussion, Wolfe jumps to an investigation of sources illuminating the founding of the city. The attempt to use consensus interpretations of a vague national culture does not work for a study of a city in the New South. Although the discussion of the assumptions of the founders is useful, the treatment suffers from the lack of a thoughtfully developed regional model. The other problem is Wolfe's chapter on associational activity. She did not consult the rich supply of theoretical perspectives on voluntary associations yielded from historians and sociologists during the last two decades. What is a good chapter might have been excellent with the incorporation of these insights.

Nevertheless, this is a strong piece of historical scholarship that presents a convincing argument against the "culture of poverty" and "colonialism" interpretations of recent southern history. Wolfe clearly shows that the decline in community cohesion and loss of a firm civic culture had at least as much to do with the common lives of Kingsport's citizens as with impersonal external forces or malefactors of great wealth. The intelligence that informs the argument and a graceful writing style make Wolfe's book a useful and welcome contribution.

GREGORY HOLMES SINGLETON
Northeastern Illinois University

REED UEDA. *Avenues to Adulthood: The Origins of the High School and Social Mobility in an American Suburb*. (Interdisciplinary Perspectives on Modern History.) New York: Cambridge University Press. 1987. Pp. ix, 306. \$29.95.

In the last few years, the evolution of the modern American high school has become one of the most important areas of research in educational history. Reed Ueda makes a major contribution to that literature by tracing the development of high school education in Somerville, Massachusetts. In his richly textured book, Ueda vividly portrays the interrelated social, political, and educational changes in Somerville from the early nineteenth to the early twentieth centuries. Strongest is Ueda's analysis of the dynamic relationship between secondary education and Somerville's economic transition from a rural village to a Boston suburb. Also engaging are detailed sections on the origins of the common school system in the town, the political and educational debates that surrounded the creation of junior high schools in the 1920s, and a superb chapter on the evolution of youth culture in the high school around the turn of the century.

Ueda's goal is to "throw light on the modern American high school in the currents of migration and social mobility generated by the industrial revolution" (p. 1). Specifically, his inquiry focuses on whether the high school reproduced the inequalities of capitalist society or served as a vehicle of social advancement for children from immigrant and working-class families. He cogently concludes that the high school did both. It helped maintain the class structure of society because wealthier families were able to send larger percentages of children to school than poorer families, but it also served as a "small window of opportunity" for working-class children (p. 221). In other words, the high school reflected but did not entirely reproduce the social order.

While these conclusions seem plausible, there are some serious problems with Ueda's approach, the prime one being his method of statistical analysis. Ueda's data set is impressively comprehensive, covering three generations of high school students (1852–1853, 1859–1861; 1871–1872, 1879–1881; and, 1899, 1900–1901) and including such variables as social class, ethnicity, home ownership, and birth order. Unfortunately, he analyzes these variables using simple two-way cross tabulations rather than with multivariate techniques, thereby treating every variable as discrete. Social class and ethnicity, for example, are explored separately, and no sense of how these variables, or any others, interact is conveyed.

Ueda's approach to so rich a data set is both surprising and frustrating. Multivariate analysis has played a major role in educational history for over fifteen years, and Ueda's data are ideally suited for such analysis. Why he bypassed the opportunity to explore this information with a powerful statistical program is a mystery. Without question, the study is weaker and its conclusions more tentative because of the methods employed.

In addition to this methodological problem, several other aspects of the book are troubling. Ueda, for example, makes no case for why Somerville is a particularly good place for a detailed case study of the high school. More important is the lack of clarity in Ueda's claims about the usefulness of the high school for social mobility in the late nineteenth and early twentieth centuries. It is difficult to ascertain from the text the real impact of the high school, since Ueda argues, at one point, that high school graduates from blue-collar homes had *less chance* of moving into white-collar jobs than did blue-collar high school dropouts (p. 166). Yet, somewhat later, he points out that students from blue-collar families who attended high school had a "strong advantage" in securing white-collar jobs when compared to blue-collar grammar school dropouts (pp. 176–77).

Since most high school students in this era did not, in fact, graduate, does that imply that attending high school, even for a relatively short time, was more helpful in moving up the social ladder than graduation? If so, does that make the high school more or less useful as an avenue for social mobility?

Such questions aside, Ueda's book is an important study. It is a welcome, thoughtful addition to the growing literature on high school development.

JEFFREY E. MIREL
Northern Illinois University

MARY ANN JAMES. *Elites in Conflict: The Antebellum Clash over the Dudley Observatory*. New Brunswick, N.J.: Rutgers University Press. 1987. Pp. xvii, 301. \$30.00.

Among the many controversies that convulsed the United States during the 1850s was the clash over the establishment and future of the Dudley Observatory in Albany, New York. The cast of characters in this controversy included leading scientists of antebellum America as well as political figures such as Secretary of the Treasury Howell Cobb and Senator Jefferson Davis in addition to various state and local politicians. Defined by the scientists involved as a clash between professionals and amateurs, the Dudley Observatory controversy has long deserved careful treatment. Mary Ann James's book shows that the situation was far more complex than originally presented and provides an important contribution to our understanding of both science and scientific philanthropy in the decade before the Civil War.

By the early 1850s, Albany had established itself as one of the centers for scientific philanthropy in the United States. The possibility of founding a world-class observatory in the city appealed to the community's political and economic elite, who coordinated the chartering of the facility by the state legislature and arranged for the funding of the observatory by Blandina Dudley, widow of Senator Charles Dudley. Members of the Albany elite served as the board of trustees for the observatory, but a scientific council was also appointed, consisting of U.S. Coast Survey superintendent Alexander Dallas Bache, Smithsonian secretary Joseph Henry, Harvard mathematician Benjamin Peirce, and astronomer Benjamin Apthorp Gould, Jr., first director of the facility. These four men were members of the famous "Lazzaroni," a group of scientists dedicated to the improvement of science in the United States in the mid-nineteenth century and committed to establishing themselves as the scientific elite of the nation. A clash between

two such imposing groups was hardly surprising. Gould's refusal to "educate" the board of trustees and the scientific council's attitude that the board should concern itself only with fund raising led to conflict that, repeating James's analogy, took on many of the characteristics of some of the finest Gilbert and Sullivan parodies. The severance of the relationship between the trustees and the council in 1858 surprised few. The Dudley Observatory began functioning the following year but under the firm control of the board of trustees.

Based on an impressive investigation of manuscript and printed sources, *Elites in Conflict* provides an important insight into the development of antebellum American science. James's most important contribution is to correct the view that the period's science was marked primarily by a clash between professionals and amateurs. The scientific council's actions in the Dudley Observatory controversy show that the Lazzaroni were as concerned with personalities and consolidating their own power within the scientific community as the trustees were with Albany politics and economics. Similarly, the board was concerned with professional and scientific considerations, desiring the Dudley Observatory to be a valuable addition to the scientific community as well as to the city of Albany. The clashes among the Dudley Observatory elites also involved questions of professional accountability and authority, especially because such questions related to the role of the expert in establishing scientific institutions supported by the public. As James emphasizes in her conclusion, this remains a crucial problem in American science. Her work supplies us with a valuable historical perspective to this important contemporary issue.

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JOHN C. BURNHAM. *How Superstition Won and Science Lost: Popularizing Science and Health in the United States*. New Brunswick, N.J.: Rutgers University Press. 1987. Pp. xi, 369. Cloth \$35.00, paper \$16.00.

John C. Burnham celebrates science as part of a liberal culture: a culture of rationality, skepticism, naturalism, and objectivity. This scientific culture is our main defense against superstition, magical thinking, irrationalism, and sensationalism. Burnham's book is the story of how nineteenth-century scientists promoted this ideal, and how twentieth-century journalists and publicists then sold it out to commercial interests. Burnham shows us scientists in the nineteenth century actively engaged in popularizing science, promoting an understand-

ing of science as part of the evolution of a progressive civilization, encouraging a new secular morality, battling mysticism and irrationality, and leading the middle class toward a new "religion of science." In the twentieth century, as scientists became specialized and preoccupied by technical matters, they stopped attending to the education of the public, stopped explaining science, and stopped writing for general audiences. They abandoned these essential cultural tasks to journalists and educators.

The media, advertisers, journalists, and, to some extent, science teachers are the villains of Burnham's story. Television has been pernicious; advertisers have corrupted the media; journalists have sold a "Gee Whiz!" version of science, and even science teachers have largely given up on the task of promoting the real content and values of scientific knowledge. Instead, they have provided isolated bits and pieces of information, no longer part of a unified system of thought and understanding but simply items in a self-indulgent consumer culture, facts without content. Science is no more the enemy of superstition; it has become a new form of superstition, a new kind of magic. The "wonders of science" and the "miracles of medicine" reinforce superstitious authority and feed commercial interests; advertisers are engaged in a remystification of the world.

Burnham's thesis is dramatic and provocative. Lucidly expressed in the first and last chapters of his book, it orients us to his detailed and complex historical survey of the popularization of science in three general fields: health, psychology, and the natural sciences. Burnham supports his thesis well in the sections on health, medicine, and the natural sciences; psychology, the ultimate popular science of the twentieth century, seems a more dubious case. One might well argue that the late nineteenth-century mania for self-improvement, so often channeled into popular science education, has in the late twentieth century become transmuted into a mania for psychological self-examination. Burnham does little with this, perhaps on the grounds that he views all forms of subjectivity with distaste. It seems odd, for example, that his chapter on the popularization of psychology fails to give even passing mention to Sigmund Freud, ignores the unconscious, and implies that all forms of psychotherapy are non-empirical and irrational "schlock."

Burnham is, however, an excellent guide to the institutions and activities of scientific popularization in the nineteenth century: the museums, lectures, and magazines that spread the "religion of science" to eager audiences. His attack on the "Gee Whiz!" school of science reporting, on misleading television advertisements, and on the

twentieth-century fragmentation of understanding is persuasive. Perhaps not all scientific popularization in the nineteenth century was so high-minded; then, too, there was fragmentary information, facts without apparent context, and some popular titillation—as in lengthy debates whether ladies could wear bloomers. And perhaps not all scientific popularization in the twentieth century is so bad. Not all of our information comes from television commercials; scientists, science writers, and historians of science, among others, do write good books, and some of them are read. Admittedly, currently popular titles such as *Chaos* and *Love, Medicine, and Miracles* may not give Burnham much comfort. But perhaps this is not solely the fault of the popularizers. Science itself has become more fragmentary, more full of curious contradictions and paradoxes, and has embraced more uncertainty. If the universe is less orderly, and the certainties of scientific authority more in question, skepticism at least seems to be alive and well.

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ARTHUR WROBEL, editor. *Pseudo-Science and Society in Nineteenth-Century America*. Lexington: University Press of Kentucky, 1987. Pp. 245. \$24.00.

They are all here, the loosely affiliated, eclectic group of pseudosciences so often identified with the nineteenth-century cultural landscape and so often regarded as colorful if misguided precursors to modern scientific ways of thinking. Arthur Wrobel and the other contributors to this collection, not content with the condescending treatment accorded the pseudosciences by Whiggish cultural critics, try to place them in their proper historical context and give them the credit they deserve for paving the way for more "modern" scientific constructs.

The volume's contributions range from Taylor Stoehr's delightful essay detailing the fascinating career of Robert H. Colver and arguing the similarity of pseudoscience and science (a major theme of the collection) to Robert C. Fuller's piece that links mesmerism with the birth of modern psychology and shows how the interest of mesmerists in numinous states made mesmerism capable of serving as an umbrella for a variety of new religious groups that flourished in America's pluralistic culture. In between are the contributions of John Greenway on the uses of electricity in the treatment of nervous diseases, of Marshall Scott Legan on the way modern medicine took the lessons of hydropathy seriously enough to incor-

porate them into regular therapeutics, of Robert W. Delp on Andrew Jackson Davis's troubled alliance with the spiritualist movement and its millennial but not Christocentric orientation, of Arthur Wrobel on phrenology's promise of a secular millennium and interest in natural laws, which explain its relevance to political theory and social science, of Harold Aspiz on the evolutionary eugenics characteristic of pseudoscientific sexuality with its emphasis on continence and the healthy body as a moral force, of George Hendrick on Washington Irving's conversion to homeopathy, and of Charles Thomas Walters on the attempt of sculptors to present human perfection through the application of phrenological principles.

Wrobel's introduction and conclusion attempt to draw together this mixed bag of essays. Admitting that on the surface the pseudosciences have little in common, Wrobel nonetheless prefers to stress the commonalities. "Of greater significance [than the differences]," he argues, "are the remarkable number of premises, methodologies, and teleological assumptions they shared and that placed them squarely in the midst of major currents of nineteenth-century thought. Their doctrines complemented the national belief that America occupied a special place in mankind's history; denied the distinction between body and mind, the material and the spiritual; gave credence to the message delivered by reformers that health and happiness are accessible to men; and presented a unified view of knowledge and human nature that seemingly accounted for the structure of nature and man's place within it. Rationalistic, egalitarian, and utilitarian, they struck familiar and reassuring chords that were pleasing to the ears of Americans" (pp. 2–3). Characterized by faulty logic and sometimes propagated by unbelievers out to make a fast buck, Wrobel holds, the pseudosciences nonetheless spoke to the spiritual and intellectual needs of nineteenth-century Americans addicted to both secular and spiritual millennialism and perfectionism. Exactly which Americans were most attracted to which pseudosciences and at which time is left somewhat vague, though Wrobel does try his best to demonstrate that the literate middle class was as interested as anyone in the liberating potential of phrenology, mesmerism, spiritualism, hydropathy, and homeopathy.

The volume's stress on the fine line that divides the pseudosciences from "real" science is well taken. Perhaps it is more often correct to view the transition from premodern to modern modes of scientific inquiry as characterized by gradual refinement and integration rather than by dramatic intellectual breakthroughs. Certainly the majority of authors in this collection attempt to make this

argument (the integration of homeopathic principles into mainstream medicine is but one obvious example), trying as they do to identify the elements of pseudoscience that worked their way into modern disciplines such as psychology and medicine. In some instances the relationship is imputed rather than demonstrated, though to some extent a lack of definition is unavoidable given the indirect measurements (magazine coverage, similarity of language, and overlap of memberships) usually employed to track intellectual developments by those who study the interstices between literature, religion, and movements in the larger culture. Perhaps the best way to characterize the style of this volume would be to borrow from a classic American studies title, *Pseudo-Science: Symbol for an Age*.

Despite the fact that several of the essays are more descriptive than analytical, overall this volume is a welcome addition to nineteenth-century cultural history. The pseudosciences deserve the seriousness with which the contributors treat them, a seriousness that scholars may display who read and absorb the message, as put by Stoehr, that "much of what we now think of as orthodoxy . . . was actually taking shape in this welter of *isms* and *ologies*" (p. 30). As Stoehr also points out, we must recognize our own age's pseudoscience, the conflation of science with technology that leads us to assume that "there is always some new discovery in the offing that will explain what is unknown and set everything to rights" (p. 30). One is tempted to invoke the old saw that the more things change the more they stay the same. Surely our society's search for health and perfection has taken forms at least as unorthodox as the pseudosciences of a century ago.

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MARK CALDWELL. *The Last Crusade: The War on Consumption, 1862–1954*. New York: Atheneum. 1988. Pp. 336. \$22.50.

Mark Caldwell explores American attitudes—medical, popular, and literary—toward the "white plague" of tuberculosis, from the death of Henry David Thoreau in 1862 until the effective introduction of drugs against the disease in the late 1940s and early 1950s. He looks for "recurring patterns and hidden themes; for the metaphors of sickness and health they generated; for what, implicitly, they may tell us about the diseases, like cancer and AIDS, that traumatize us today" (p. 5). The "War on Consumption," he concludes, "perpetrated its incidental cruelties and follies," but it

ultimately achieved a "clinical victory." In so doing, it "told us more than the truths of biology. It told us who we were" (p. 288).

In developing this argument, Caldwell necessarily draws on the excellent work of other scholars. Following the lead of Susan Sontag, whose *Illness as Metaphor* (1977) focused on artistic renderings of tuberculosis and cancer, he devotes considerable space to literary representations of tuberculosis. Following Rene Dubos and Jean Dubos (*The White Plague: Tuberculosis, Man, and Society* [1952]), he includes material on social aspects of the disease. And following Richard Shryock (*National Tuberculosis Association, 1904–1954* [1957]), he offers a chapter on the role of Herman Biggs and other public health reformers during the Progressive era. He writes assuredly about medical and scientific developments, including surgical approaches to the disease and the development of antibiotics.

Caldwell emphasizes the change in attitudes toward tuberculosis since the mid-nineteenth century. When Thoreau died of it in 1862, many people still considered it to be an angelic visitation that often afflicted sensitive, artistic people. By 1900, however, it was widely perceived as the dangerous disease that it was—America's number one killer. Then, and for many years thereafter, tuberculosis was the dread disease of America.

Caldwell focuses on the rise of the sanatorium movement in the United States. Indeed, approximately half the book concentrates on the role of sanatoriums. Caldwell's lead character is Edward Livingstone Trudeau, who opened a bungalow for tubercular patients in Saranac, New York, in 1885 and soon presided over a sprawling complex of institutions there. The dominant force in the sanatorium movement, Trudeau also was a leader of the National Tuberculosis Association.

Caldwell's fascination with sanatoriums tends to distort the nature of the crusade against tuberculosis (he devotes an entire chapter to describing the town of Saranac). But it enables him to describe fully the lives of patients—always a small minority of Americans who developed the disease—at these remarkable institutions. He argues convincingly that most sanatorium patients became contented at scenic places like Saranac, and that the rest cures featured there were remarkably unintrusive compared to the contemporaneous high-tech developments in "modern" medicine. (Many sanatorium patients in the 1920s and 1930s, however, underwent pneumothorax, a sometimes painful and essentially worthless surgical procedure.) But Caldwell makes no great claims for the effectiveness of sanatoriums. The "sanatorium regimen," he says, "could claim some plausible successes; but it wasn't so much science as

art and folklore, adapted as far as possible to take advantage of what science had revealed about the disease process." By 1920, "the average sanatorium was closer to a summer camp or a health resort than it was to a modern hospital" (p. 249). By the 1950s, their therapeutic value unproved, sanatoriums virtually disappeared.

When Caldwell writes about sanatoriums, he is persuasive. Elsewhere, he is less convincing. Sometimes, he includes colorful material (as concerning Bernarr MacFadden) that seems only remotely relevant to his main themes. His treatment of the Biggs and the public health reformers is comparatively brief and strangely hostile. When he turns to literary matters, he becomes tedious. He devotes three pages of analysis to a pamphlet issued by the Metropolitan Life Company and three more to commentary on playlets that appeared in the tuberculosis association's magazine, *Journal of Outdoor Life*, in 1915. Henry James's *Wings of the Dove* receives ten pages of textual analysis. Such scattered sources do not explain much about popular attitudes. Moreover, Caldwell becomes needlessly wordy and does not make it wholly clear what these sources have to say about American responses to the disease.

Still, this is a worthwhile book. Most of the chapters are written well: medical aspects are explained clearly; and the sanatoriums receive full and balanced description. *The Last Crusade* deserves a place among recent social histories of health and disease in America.

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DAVID N. LIVINGSTONE. *Nathaniel Southgate Shaler and the Culture of American Science*. (History of American Science and Technology Series.) Tuscaloosa: University of Alabama Press, 1987. Pp. xiv, 395. \$32.95.

In the last decades of the nineteenth century, Nathaniel Southgate Shaler was admired not only as a professor of paleontology and geology and dean of the Lawrence Scientific School at Harvard University but also as a prolific writer on a variety of subjects. Works on geology, geography, history, man and nature, race and immigration, and even drama and poetry flowed from his pen. In this study, David N. Livingstone discusses the various aspects of Shaler's thought and work within the general context of late nineteenth-century intellectual trends.

In his introduction, Livingstone emphasizes that Shaler's work as "a founding father of American academic geography" mirrored currents of nineteenth-century thought in geology, theology, biol-

ogy, and sociology and that his work exhibited "the same fundamental reorientation in the conception of the relations between nature, humanity, and God that was necessitated by the Darwinian revolution" (p. 6). He also argues that Shaler provides "a particularly appropriate lens for focusing on the context of geography's modern renaissance in the United States" (p. 7) and that Shaler's "synthesis of the prevailing scientific-religious ethos as the context for his geographical writings" provides "a more general perspective on American attitudes to humanity and nature in the late nineteenth century" (p. 9).

The consistent theme of Livingstone's book is the depiction of Shaler as a transitional scholar. Livingstone argues that writers have often overemphasized the clash between the new Darwinian science and the old religion and that Shaler well illustrates the efforts of scientists in the late nineteenth century to synthesize old and new views. Shaler was a student of Louis Agassiz, but he also was attracted to Darwin's theories of evolution. Livingstone sees Shaler as one of the American neo-Lamarckians, although he emphasizes that Shaler cannot be neatly categorized.

Livingstone points out in his introduction that his book "is structured rather differently from conventional biography" (p. 9). The short introduction is followed by a second chapter of some forty pages devoted to a brief examination of the main facts of Shaler's life from his birth in 1841 to his death in 1906. The remaining seven chapters are devoted to thematic examinations of Shaler's writings on a variety of topics—evolution, natural theology, race, environment, conservation, geology, education. Within the chapters, there are subheadings as a further guide to topics. For example, in the chapter on Shaler's racial views, among the subdivisions are brief sections devoted to "The American Negro," "The North American Indian," "The Jew," and "The European Peasant."

Although Livingstone has delved in detail into Shaler's thought and writing and the background of late nineteenth-century intellectual history, he has not succeeded in writing an effective biography of Shaler. The discussion of Shaler's work is to a large extent divorced from Shaler's own life and background, and there is little explanation of how his ideas might have evolved over time or of how Shaler made his personal compromises with the flood of influences pressing on him. The author has not solved the problem of weaving his themes within a temporal context and adds to the reader's difficulties by the use of an opaque writing style. This is more an extended discussion of Shaler's

writings in their late nineteenth-century setting than it is a biography of Shaler.

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RIMA D. APPLE. *Mothers and Medicine: A Social History of Infant Feeding, 1890–1950*. (Wisconsin Publications in the History of Science and Medicine, number 7.) Madison: University of Wisconsin Press. 1987. Pp. xv, 261. Cloth \$39.50, paper \$16.25.

In the late nineteenth century, mothers seeking advice on the feeding and care of infants relied on traditional knowledge gleaned from a female network of relatives and friends. Sixty years later their great-great-granddaughters routinely turned, instead, to the medical "experts" whose learning they equated with science and progress. In this monograph, rewarding to read, Rima D. Apple analyzes how and why dramatic changes took place in infant-feeding patterns in the United States in the decades between 1890 and 1950 and investigates how women used available information at any given period in order to improve the quality of the domestic care they provided.

Breast-feeding was the norm in 1890, and mother's milk was regarded as the natural, most desirable infant nourishment. When variations in quality or quantity demanded supplements or substitutions, physicians recommended bottle-feeding using a simple formula of modified cow's milk. (Class-conscious, they rejected wet-nursing, alleging that infants acquired undesirable moral and physical characteristics directly from the nurse's milk.) Available alternatives were chemically prepared soluble milk substitutes such as those developed by Henri Nestlé, Gustav Mellin, and Gail Borden and manufactured by proprietary infant-food companies. Widely advertised, these products and the simple accompanying directions enabled women of all classes to bottle-feed their infants without consulting physicians.

Apple meticulously details how the "science" of artificial feeding developed in the Progressive period. Funding of bacteriological researchers, nutritional and dietary studies, and better medical education and the age's characteristic faith in the abilities of "experts" all contributed to this trend. Medical researchers (who looked askance at the infant-food manufacturers) established an elaborate theoretical basis for scientific feeding, which included a percentage system of formula preparation based on a physician's prescription that was designed to take into account each infant's unique and changing needs. The use of elaborate formula

charts, milk laboratories, or home modification of milk further complicated the system. Apple argues that, as clinicians, general practitioners used the medical theories researchers formulated in order to add the supervision of infant feeding to family practice. In a scenario strikingly reminiscent of the medicalization of childbirth, family physicians were motivated both by a desire to promote their own economic interests and by a humanitarian impulse to reduce the unacceptably high infant mortality rate they associated with gastrointestinal infections. Physicians eventually succeeded in persuading the public as well as infant-food manufacturers, with whom they developed a mutually profitable relationship, that "infant feeding was a medical function" (p. 54).

The second half of this study focuses on the emergence of the ideology of "scientific motherhood." This concept retained the maternal role central to the earlier cult of domesticity but increasingly stressed the need to rely on the advice of scientific and medical experts. Women anxious to provide their babies with the best possible care willingly accepted and even actively solicited medical supervision of infant feeding. One consequence was that by 1950 breast-feeding was no longer the norm. Mothers approved of and found bottle-feeding satisfactory and "generally accepted it as standard infant feeding practice" (p. 166).

This balanced, well-documented, and cogently argued study makes excellent use of a wide array of written and oral sources, ranging from women's magazines, medical journals, government publications, advertisements, and child care manuals to personal interviews with contemporary mothers. Apple skillfully sorts the prescriptive literature from the descriptive and demonstrates the impact each had on the other. Students of medical history, social history, and women's studies as well as the general reader will find Apple's work a unique, valuable addition to the literature.

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MARIS A. VINOVSIS. *An "Epidemic" of Adolescent Pregnancy? Some Historical and Policy Considerations*. New York: Oxford University Press. 1988. Pp. xix, 284. \$24.95.

Maris A. Vinovskis asks provocative questions in his imaginative new book about pregnancy among American adolescents in the past and present. The book blends historical and quantitative analyses with ethnographic description of contemporary policy issues based on Vinovskis's experiences as deputy staff director of the U.S. House Select Committee on Population from 1977 to 1979 and

later as a consultant to Marjory Mecklenburg and the Office of Adolescent Pregnancy Programs in the early 1980s. He begins by reevaluating whether there is an epidemic of adolescent pregnancy. Vinovskis answers yes, in a sense, but not because the actual number of babies born to adolescents has increased. "[S]ince 1970 a substantial decrease in the number of births as well as in the birth rate for females ages fifteen to nineteen has occurred" (pp. 25–26). What has taken place is an increase in childbearing rates of younger girls, ages ten to fourteen, as well as a large increase in the rate of out-of-wedlock births among adolescent girls (pp. 27–28). Much of the rest of the book details the uninformed decision making of policy makers who neither understood some of these relatively simple facts about adolescent pregnancy nor were willing to reconsider some of the assumptions, liberal or conservative, underlying attempts in the late 1970s and early 1980s to develop federal programs addressing the needs of pregnant teenagers and their children.

Later chapters discuss how the federal Office of Adolescent Pregnancy Programs was started during the Carter administration. Controversies surrounding attempts to keep this program alive when the Reagan administration began cutting and shifting funds to block grants are also described. Vinovskis assesses much of the significant social science research used to promote federal programs for pregnant teenagers. His critiques of the statistics and underlying assumptions of these studies are models of clarity.

Although it is impossible to detail all of the useful points he makes, there are three especially noteworthy discussions about contemporary adolescent pregnancy. First, Vinovskis shows how useful it is to examine adolescent pregnancy by age grouping. Younger teenagers are bearing an increasing number of the children born to adolescents. He suggests that the Reagan administration might have had more success with its proposed parental notification rule if it had paid more attention to this shift in teenage distribution of the adolescent fertility rate. Second, the author proposes a serious rethinking of the trend toward discouraging teenage girls from marrying the fathers of their babies. He cites evidence that a majority of teenage marriages are intact five years later (pp. 178, 243n). Vinovskis also points out that encouraging increasing involvement on the part of fathers might aid the "early development of the new-born child" (p. 173). Third, he touches on the roles of the media and professional groups in influencing the discussion of sensitive political issues like this. These groups have access to scientific information but often use it selectively to communicate their own viewpoints, supporting

their own agendas, which may or may not coincide with majority public opinion.

Two chapters deal with historical issues. In analyzing data about age at menarche and marriage, Vinovskis concludes that in the past Americans were little concerned with adolescent childbearing because it happened much less often. He suggests that, if adolescent pregnancy was controlled in the past, it might be in the present or future as well. The book also succinctly summarizes historical changes in the father's role. Our contemporary view that a mother is generally best and "naturally" suited to child care is shown to be an outgrowth of nineteenth-century changes in men's and women's roles. These historical analyses provide a broader background for analyzing current problems and governmental responses.

Included in the book are descriptions of the microprocesses of specific legislation enactment under the Carter and Reagan administrations. Herein lies the one troubling part of this quite informative volume. Although Vinovskis identifies the positions he held as a consultant and as a deputy director of a House committee, and describes a few of his opinions on the issues (pp. xiii–xvi, 234–35n), he says that he was primarily viewed as an outsider, because he was not a decision maker and therefore could maintain "detachment" (p. xvi). Yet, to anyone reading this book, he was far more of an insider than he may realize, which raises special questions about his role and the information to which he had access. I assume that an insider perspective allows more detailed reconstruction of the politics of adolescent pregnancy than would otherwise be possible. Yet this perspective also entails a loss of objectivity. There is need for a more thorough description of the ethnographic sources—interviews, conversations, and close observation of legislative maneuverings—that he uses. As historians increasingly do research with overt policy implications and use observational or ethnographic techniques, they will have to consider the practical, ethical, and reflexive issues that arise for all participant-observers. This book may be viewed as a forerunner of a new type of historical ethnography in which historical and ethnographic research techniques, all with a policy focus, are combined. Yet more attention needs to be given to the ethnographic techniques used and their implications.

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WILLIAM G. ROTHSTEIN. *American Medical Schools and the Practice of Medicine: A History*. New York: Oxford University Press. 1987. Pp. xii, 408. \$29.95.

In his new book, William G. Rothstein, a sociologist who published *American Physicians in the Nineteenth Century: From Sects to Science* (1972), traces the history of American medical education from its earliest days to the present. The period from 1750 to 1900 is covered in the first third of the book, the period since 1900 in the remainder. The major contribution of this work is the detailed analysis of the period since 1950, which has not been studied by medical historians in anything resembling the depth provided by Rothstein. During these years, there was an extensive expansion of medical and scientific research, a rapid growth of medical school enrollments, and questions pertaining to the number and distribution of general practitioners as well as specialists.

The first section of the book demonstrates that medical schools grew stronger during the first half of the nineteenth century as they displaced the apprenticeship system. Rothstein indicates that private schools and classes provided competition for the medical colleges and that medical students began to influence changes in medical education. Then Rothstein describes education that took place in a hospital setting, education of medical students in amphitheater and ward rounds, of house officers (residents), and nurses. Because hospitals were service oriented, service needs took precedence over education, resulting in deficiencies in hospital training. Rothstein also describes the development of polyclinic medical schools, which provided postgraduate training in the specialties. This type of institution was eventually replaced by residency training. By the end of the century, according to Rothstein, clinical faculty members had become the most influential of the groups that determined the content of medical education.

Rothstein describes a major change in the power structure of medical schools, during the period from 1900 to 1950, largely because new sources of funding came from government and endowments. This resulted in the use of full-time clinical professors and enabled the basic science faculty members to pursue their own interests rather than those of medical students. Together this widened the differences between the interests of clinical and science faculties. By 1950 there had been a major change, with students having to compete for admission and tuition and fees providing a minor part of the financial support needed by the colleges. Full-time clinical faculty came to dominate the schools, and university hospitals grew because of the benefits they provided to the faculty rather than because of their contribution to teaching or research.

Since 1950, according to Rothstein, medicine has changed in many ways. The elderly and poor

receive much more care than they did previously, and more cases of chronic and degenerative diseases require treatment. The increase in hospital and office care (rather than house calls) made it possible to make more efficient use of time and instruments but reduced the physicians' understanding of social and family factors in illness. Specialization further reduced the physicians' focus, and increased wealth "may have affected their values" (p. 206).

Rothstein has provided an important study, a synthesis based on an extensive reading of published sources. In the process, he provides a gold mine of statistical information and suggestions on the meanings of that information. Unfortunately, throughout the book his conclusions are based on supposition and have not been supported by either quotations or documentation. From the perspective of the medical historian, this study should become a valuable point of departure for persons interested in studying the recent development of medical science and education. It is useful to compare Rothstein's work with Paul Starr's *The Social Transformation of American Medicine* (1982), another book by a medical sociologist. Starr's prize-winning study was a marvelous synthesis, offering very little that was "new" to specialists but putting together the secondary literature into a well-written and meaningful description of social changes in medicine from the nineteenth century into the twentieth. Rothstein's book, on the other hand, is based on far more extensive research, covering secondary works as well as published primary sources, and it provides a useful compendium. Many of his conclusions, however, are largely unproven (although perhaps correct). In the long run, it is likely that Rothstein's book will have a far greater influence on the field of medical history, as his suggestions should encourage a great deal of research, which should expand our knowledge and understanding of the evolution of twentieth-century American medicine.

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J. WORTH ESTES and DAVID M. GOODMAN. *The Changing Humors of Portsmouth: The Medical Biography of an American Town, 1623-1983*. Boston: Francis A. Countway Library of Medicine. 1986. Pp. xv, 363. \$19.95.

As its subtitle attests, this book is a "medical biography" of an American town. Authors J. Worth Estes and David M. Goodman, respectively physician and Ph.D. historians, have set about writing a history of American medicine from a local perspective. Their choice of Portsmouth,

New Hampshire, for this study reflected several considerations: the volume was first conceived as part of a centennial commemoration of the Portsmouth Hospital, yet soon expanded in scope as the authors found the town to be exceptionally rich in primary source materials concerning its medical history, including personal papers, institutional records, and newspapers. Blessed with detailed archives, the authors saw an opportunity to write a more ambitious history of American medicine from the seventeenth to the twentieth centuries. The fact that Portsmouth was neither a large city nor a major center of medical education and research struck them as an advantage rather than a disadvantage: it gave Estes and Goodman the opportunity to tell "the story of average American medicine—if there is such a thing—uninfluenced directly by academic politics or by the kinds of professional efforts that are peculiar to medical schools, such as scientific research" (p. xii).

Taking this approach, Estes and Goodman predictably do not present any radically new findings about American medicine: rather, their work retraces familiar lines of development from a community perspective. The results are consistently fresh and entertaining. Rooting the evolution of ideas and practices in a local context gives new interest and immediacy to the large-scale processes of therapeutic change, professionalization, and institutional development that shaped the history of American medicine.

The authors recount Portsmouth's medical biography as a series of overlapping stories: the chapters are thematic in focus and chronological in organization, beginning with a broad overview of "health, disease, and doctors" in the town's first two centuries. Chapters 2 and 3 document the medical world of the nineteenth and early twentieth centuries. In Chapter 2, prosopographies of Portsmouth practitioners are effectively used to enliven the familiar battles between regulars and irregulars, as in the 1830s contest between Francis Cogswell, a homeopath and a Democrat, and the town's Whiggish regulars. Chapter 3 recounts the history of the Portsmouth Medical Society, which was formed in 1879 as an effort to form "a united front," both intellectually and economically, against doctors' competitors. The authors make good use of the society's transactions to examine the general practitioners' reaction to new medical techniques and concepts such as anesthesia, anti-sepsis and asepsis, and bacteriology.

Several lengthy chapters document the history of the "Cottage Hospital" founded in 1884, later known as the Portsmouth Hospital, thus continuing the authors' portrayal of medical life into the twentieth century. Given that this volume was intended to mark the hospital's centennial, it pres-

ents a remarkably trenchant critique of its shortcomings, particularly the trustees' and medical staff's resistance to change. The authors provide a careful account of the circumstances that led to a bitter strike by nurses in 1980, and the trustees' decision to sell the hospital to a for-profit hospital chain, the Hospital Corporation of America, in December 1983. Given that most histories focus on institutions that excel, it is refreshing and stimulating to read a thoughtful analysis of why one hospital did not work so well.

The authors also include a brief but interesting chapter on the public health movement in Portsmouth. The volume concludes with a number of interesting appendixes, including material from bills of mortality, lists of regular and irregular practitioners, and operations performed and diagnoses assigned at the Cottage Hospital. In the text itself, there are reprinted fee bills and a variety of useful charts. While the book dispenses with footnotes, it contains extensive bibliographic notes at the end of each chapter.

In general, this book is a valuable compendium of interesting information and thoughtful commentary on "average American medicine," particularly in the nineteenth century, which will appeal to specialist and non-specialist alike. Given the dearth of useful surveys of American medicine, its appearance is all the more welcome. *The Changing Humors of Portsmouth* should be read along with all the many texts celebrating the rise of modern medicine to give a more balanced view of the development of American health care.

NANCY TOMES
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JAKE W. SPIDLE, JR. *The Lovelace Medical Center: Pioneer in American Health Care*. Albuquerque: University of New Mexico Press, 1987. Pp. xii, 217. \$27.50.

Using his recently published *Doctors of Medicine in New Mexico: A History of Health and Medical Practice, 1886–1986* as a backdrop, Jake W. Spidle, Jr., presents a chronicle of Albuquerque's Lovelace Medical Center. In doing so, he draws primarily on a wealth of documentary sources, several relevant archival collections, and interviews with individuals associated with Lovelace over the years. The author's stated intention is twofold: "to honor this important Southwestern institution . . . and the men and women who envisioned its service, charted its course, and labored to fulfill its goals . . . [and] to fix the history of the Lovelace institution within the general history of the community and region it has served and to integrate its

development within the broader history of American medicine in the twentieth century" (p. x).

The story begins in the early 1900s when two young physicians suffering from tuberculosis, William Randolph Lovelace from Missouri and Edgar T. Lassetter from Georgia, migrated to east-central New Mexico Territory with a host of other "lungers" seeking a cure in "nature's sanatorium." Meeting in Albuquerque during World War I, the two merged their practices in 1919 and established a formal partnership in 1922, the date used to mark the founding of the Lovelace Clinic. Between 1925, when the partners hired a salaried associate, and 1941, when the clinic's staff numbered fourteen board-certified physicians occupying an entire floor of downtown Albuquerque's First National Bank building, the focus of practice shifted from general medicine to specialized care. By World War II, the Lovelace Clinic (claiming the Mayo establishment as its model) had become well-known and had brought advanced medicine to the far Southwest.

After the war, leadership of the institution passed largely from lifelong bachelor "Uncle Doc" Lovelace to his Harvard-Mayo trained nephew and protégé, William Randolph Lovelace II, who envisioned an expanded role for the clinic. In 1947, the Lovelace Foundation for Medical Education and Research embarked on a quest for greater achievements in clinical medicine and contributions to research in aviation medicine and medical education, an initially successful effort that faltered in 1965 with Randy Lovelace's death in a plane crash. The ensuing years of financial and administrative crisis resulted in the creation of the Lovelace-Bataan Medical Center in 1969 and, subsequently, the Lovelace Center for the Health Sciences in 1973. During the following decade, the medical center underwent expansion including a reorientation to primary care in the clinic, a pioneer health maintenance organization, and a chain of satellite clinics to serve Albuquerque and environs. In 1984, the Lovelace Trustees and the Hospital Corporation of America formed the Lovelace Medical Center, Inc., which in turn consolidated with a division of Equitable in 1986 to form Equicor, a new giant in the field of managed health care. The short history of the Lovelace clinic is simply an epitome of the astonishing transformation of medicine and medical practice in twentieth-century America.

It is a good story and worth the telling, but unfortunately it comes across as a "booster" history in which the author misses no opportunity for laudatory hyperbole. Flawed as it is by stylistic excess, however, this is an altogether worth-

while contribution to the history of American medicine.

JOHN H. ELLIS
Lehigh University

TIMOTHY C. JACOBSON. *Making Medical Doctors: Science and Medicine at Vanderbilt since Flexner*. (History of American Science and Technology Series.) Tuscaloosa: University of Alabama Press. 1987. Pp. xiv, 349. \$26.95.

In many ways, this is the best one-volume history of an American medical school yet to appear. Thoroughly researched, unusually well-written, it traces the modern history of a major Southern medical school against the background of national currents in science, medicine, and philanthropy. The book begins with the creation of the new Vanderbilt School of Medicine in 1925 through the combined efforts of Chancellor James Kirkland, Abraham Flexner, and Dean G. Canby Robinson. Each of these men represented an important strand in the fabric of modern medical training in America. Chancellor Kirkland, German-trained, fiercely energetic, imbued with a vision of what a great university should be, stands for dozens of other university leaders in bringing scientific medicine fully into the university. Flexner is sympathetically portrayed as the highest type of foundation official, intelligent, persuasive, and demanding, and the man responsible for Vanderbilt's medical school becoming the largest recipient of Rockefeller Foundation largesse. And Robinson was the last of the medical generation who sat at the feet of William Osler and Franklin Mall, spent many months in German clinics and laboratories, and who then brought scientific medicine to American classrooms. At Vanderbilt, he constructed a medical school closely modeled on the German-Hopkins example, with wards, laboratories, and classrooms arranged to promote maximum interaction among research, teaching, and practice.

The school that Robinson built reached the apogee of its influence and success in the 1920s and 1930s. Able young medical scientists were recruited—Barney Brooks, Ernest Goodpasture, Alfred Blalock, John Youmans—and won national and international renown. A "fluid research fund" was negotiated with the Rockefeller Foundation to sustain original research beyond the limits of departmental budgets. Increasingly, however, foundation interest waned and the school found itself forced to do more and more with less and less. World War II had a disastrous effect on enrollments, the number of faculty and staff fell sharply, and the teaching load rose dramatically. Research

of interest to faculty members had to be abandoned in the face of patriotic calls for larger teaching loads and war-time research projects. The teaching hospital, so carefully designed to serve the needs of Vanderbilt's faculty, was forced to accept more and more paying patients, regardless of their usefulness for teaching purposes.

Gradually, new social forces shaped Vanderbilt's future development as they did medical schools across the country. The public's faith in science, whetted by the war, led to the creation of vastly improved instruments for fostering medical research in the form of the National Institutes of Health. This new dependence on governmental support changed fundamentally the power relations between groups in the faculty, affected the medical curriculum, and guided research into avenues likely to be productive of immediate public benefits. Finally, as national support for research began to level off in the 1970s, it was replaced by a great surge of consumer interest in the products of modern medical science. Group insurance and federal programs for the aged and indigent brought a huge expansion of medical care provided in hospitals or "medical centers." A part of this money was siphoned off for teaching and research purposes through professional practice plans. By the end of the 1970s, more than a third of the revenues at the Vanderbilt School of Medicine was coming from patients.

Timothy Jacobson has described by way of a case study of Vanderbilt the tortuous route taken by medical education since 1925. At times critical, constantly raising questions about the effect of the many changes, he has written a unique account of a modern medical school from Flexner to Medicare. If the book has a weakness, it is a tendency to be repetitious, which causes the narrative to move slowly in a number of places.

THOMAS N. BONNER
Wayne State University

HAZEL V. CARBY. *Reconstructing Womanhood: The Emergence of the Afro-American Woman Novelist*. New York: Oxford University Press, 1987. Pp. 223. \$19.95.

It's an old quest to discover where truth may lie. Can it abide in scientific histories, "verifiable" presentations? Can it emerge from imaginary renderings, some of which draw heavily on historical happenings, on common "verifiable" situations? It's another old quest to render usable the results these approaches achieve.

Hazel Carby provides in her book a sensitive, sophisticated engagement with both quests. And she furnishes basic information—in Gerda Lern-

er's words—of an "obscured, neglected and distorted" segment of American life. Into the picture, in rich ramification, she introduces real experiences of black women leaders, of black women intellectuals, of black women literary artists of post-Civil War America.

Sometimes Carby writes as historian or political commentator, describing organizational activities, speaking engagements, journalistic endeavors of black women whose names we only begin to know; sometimes she writes as literary critic, distilling themes of nearly forgotten, unprofitably forgotten, black women novelists. From Anna Julia Cooper's ringing yet compassionate denunciations of sexist-racist tragedies to Ida B. Wells's powerful essays analyzing lynch mobs to Frances Ellen Watkins Harper's fictional depiction of the proud, tortured character Iona Leroy to Pauline Elizabeth Hopkins's intense portrayal of the fictional figures Anson Pollok, a slaveholder, and John Langley, a repressive young black patriarch, Carby highlights motifs touching the shaded actions of real white and black American women and real black and white American men.

From this difficult assignment of uncovering truths as black women perceived them and as they expressed them by interweaving their expositions and their fictions, Carby does extract themes central to American life. Some of them: the imagined menace of black rape of white women and the real tragedies of the lynching of black men and black women that resulted from white fears of an effective black political presence; the often covert, but sometimes open, racism of white women that denied black women a voice in suffrage movements and other mutually needed endeavors; the early address of these issues by black women's organizations; imperialist colonizations within the nation that assured social deterioration; the consequences of racist assertions of white supremacy that redounded to the disadvantage of all.

Like the black novelist, social historian, and public protagonist Charles Waddell Chesnutt, who was a contemporary, the black women writers of this study strove in their public lives and in their fictional output to use their talents as a means of attaining social justice. But they wrote at the highest tide of racial antagonism. Like Chesnutt, they found no way to accomplish what they wanted to do. As women, and as black women, they only glimpsed the mountain top. Perhaps, through their efforts, they lighted the way.

But Carby succeeds in bringing to life their struggles toward their vision. She has skillfully conceived and artfully written an honest, searching book of enormous value.

FRANCES RICHARDSON KELLER
San Francisco State University

GEORGE DEKKER. *The American Historical Romance*. (Cambridge Studies in American Literature and Culture.) New York: Cambridge University Press. 1987. Pp. viii, 376. \$34.50.

This, to borrow an adjective to which George Dekker has frequent recourse, is a rich book. It is rich in its subject, in the range of novelists it covers, in its subtlety and diversity of interpretation.

The title literally presents the subject, except that Dekker gives much attention also to Sir Walter Scott, as the virtual founder of the kind of historical romance Americans have written. The controlling sensibility for the historical romance in Dekker's presentation was, and remains, the sense of irony. That is because its setting, typically, is at the passing or embattlement of an older way of life, when traditional loyalties and beliefs confront a more matter-of-fact rationality and morality: an ironic moment of opposing psychologies, neither of which understands the other, or the inadequacies of its own codes, or the ways that events are going to invert its purposes and apparent victories. And this country, which is sometimes thought to have scarcely a history at all of lost and passing things, lives a story of passings. Nathaniel Hawthorne's Puritans, James Fenimore Cooper's frontier, Willa Cather's Nebraska, the South that haunted William Faulkner and Allen Tate: these begin rather than exhaust the list.

From time to time, Dekker as he arrives at some turning in his study will try a listing of the properties that the historical romance puts into opposition. Roughly, they amount to this: feeling, spontaneity, and a reverence for mystery belong to besieged cultures; order, dry reason, and an efficient sort of practicality associate themselves with a demystifying modernity. In the thickness of experience that good fiction attempts to render, these oppositions will not stay in place. Thus the Gray Champion in Hawthorne's story of that name stands both for a stern old Puritanism and for a renewal; *The Scarlet Letter*, if simplified in a way that the tale ultimately resists, in putting the celebrative at poles from Puritanism would rank the Puritans among the moderns of other romances. George Posey of Allen Tate's book *The Fathers* is a modern outsider to Major Buchan's antebellum Virginia; but his temperament is, in a way that is unsatisfactory to him, passionate measured against the ordered and formal feelings of the major's society.

The historical profession, which has learned to wince at any declaration that history teaches thus-and-such, and to doubt that history has anything particularly definable to teach, can acknowledge quite readily that history at any rate does teach

irony. Irony is also among the most useful perceptions for a novelist of any sort who wishes to trace the moral and emotional development of individual character. And the commands of feeling and reason, of spontaneity and restraint that Dekker's novelists define are in fact essential, conflicting, and intricately combined imperatives within Western and perhaps any civilization, not only in historical sequence but always and simultaneously. The historical romance, then, offers a rare chance for consciousness to encounter and argue with itself, and Dekker's study is an excellent entrance into that argument.

THOMAS R. WEST
Catholic University of America

DAVID GREEN. *Shaping Political Consciousness: The Language of Politics in America from McKinley to Reagan*. Ithaca: Cornell University Press. 1987. Pp. xii, 277. \$29.95.

This book has two purposes. The text shows how the control of political language has distorted American politics. The subtext seeks to rehabilitate a political tradition that was marginalized by what David Green calls linguistic reification. Five labels have dominated political discourse, Green shows, since the emergence of the United States as an industrial power. The debate between individualists and paternalists was succeeded, with William McKinley, by one between conservatives and radicals. That was in turn displaced by the Theodore Roosevelt/Herbert Croly distinction between progressives and reactionaries, which gave way to a contest of liberals against conservatives, first during World War I (here Green has made an important discovery) and then with the New Deal. The liberal/conservative opposition was itself overshadowed, beginning with the cold war and climaxing under Ronald Reagan, by the contest between anticommunism and communism.

One set of actors seized the dominant label in each period, trapped its adversaries in positions they did not want to assume, and projected onto opponents attitudes to which the opponents themselves were trying to call attention. Thus individualism buried the corporate character of industrial life; conservatism hid innovation in domestic and foreign policy; progressivism justified a statist assault on individual, voluntary action; liberalism disguised coercion as freedom and confiscation as generosity; and anticommunism obscures the collectivist alliance between the state and giant corporations.

Academic historians and political scientists, according to Green, became custodians of the dom-

inant vocabulary. By accepting its labels as descriptions of real entities, they used the reified categories as research tools instead of subjecting them to investigation. Green's main target is the liberal/conservative continuum, used by politicians and scholars alike to valorize a supposed center against extremes and to identify liberalism with a swollen state mobilized for war.

Green wants to save American history from its academic custodians not simply for reasons of disinterested scholarship but also (his subtext) to replace the left/right division by one between voluntary individual action and corporate/state power. His praise of the private realm does not celebrate the corporation, but, once he gets to the twentieth century, Green's central target becomes the state. He therefore treats McKinley more kindly than Theodore Roosevelt, Herbert Hoover more kindly than Franklin D. Roosevelt, Robert Taft more kindly than Harry Truman. The real heroes of this book are not Republicans, however, but radicals—Hamlin Garland against the corporations, Robert La Follette against Theodore Roosevelt, Randolph Bourne against Woodrow Wilson, John T. Flynn against FDR and Harry Truman; and Martin Luther King, Jr., and Murray Rothbard against the Great Society and Ronald Reagan.

This is not the usual place from which to write American history, and it produces lively, unsettling, and often brilliant interpretations. But Green must not simply explain the displacement of his tradition but constitute it retrospectively as well, and that herculean task leads him to assign to language a weight it cannot bear. Time and again, the failure to see through political labels explains political defeat. Conflicts of interest among those who should have seen their shared values are as underplayed as is the significance of economic and political power for who wins and who loses. Yet Green acknowledges that Garland's effort to reapropriate individualism was already doomed by the late nineteenth century, when this book begins, and that loss surely had something to do with corporate domination. Control over words has extralinguistic sources; insight alone does not supply power.

To explain why critics of the trusts like Richard Ely and John R. Commons accepted corporate language, Green points not to the corporate power they faced but to their European training. That whiff of xenophobia gives too much credit to the academy. Green's concerns, moreover, are not so disconnected from European debate as he might think. Just as his defense of voluntary action has links to the contemporary German rehabilitation of civil society (whose meaning has shifted from private marketplace to public, communica-

tive space), so his apotheosis of discourse parallels the French influence in American universities. Green takes his epigraph from Stuart Chase, not Jürgen Habermas or Jacques Derrida, but whether in down-home American or fancy European, turning to language has its limits. The wish to recover a real America buried under corporatist and European labels enables more historical insight than the immigrant-rooted scholars who came of age with the New Deal could imagine. But, in blaming words for the defeat of American voluntarism, David Green is attributing to his opponents the linguistic reification to which he himself has been driven.

MICHAEL ROGIN
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Berkeley*

CHARLES W. CALHOUN, *Gilded Age Cato: The Life of Walter Q. Gresham*. Lexington: University Press of Kentucky, 1988. Pp. viii, 280. \$28.00.

An up-to-date biography of Walter Q. Gresham has been long overdue, and historians will welcome Charles W. Calhoun's admirable study of one of the Gilded Age's most enigmatic public figures. With the exception of the useful but hagiographic two-volume work provided by his wife and son early in this century, accounts of Gresham's life have been sketchy, incomplete, and usually have focused on his brief tenure as secretary of state in the second Grover Cleveland administration. Calhoun has not entirely broken this pattern, devoting nearly one-half of his book to an analysis of Gresham's foreign policy and his record at the helm of the State Department. But he has probed deeper in search of the elusive Gresham *persona*, by exploring the future secretary of state's coming of age in antebellum Indiana, his youthful political flirtation with the 1850s Know-Nothing movement, his exemplary tour of duty during the Civil War for which he was awarded the rank of brevet major general, and his distinguished judicial career on the federal bench in the postwar years.

The enigmatic quality of Gresham's public life raises intriguing questions. For example, what was the basis of his sustained hold on the public mind amid the corruption and greed of the Gilded Age? How did this independent-minded, highly principled public figure, who won a single elective office in his political career and that early on in 1860, who recoiled at the indignities of seeking voter approval on the campaign trail and confessed, "I haven't the cheek to stand up and contend for a seat in the U.S. Senate," have such broad appeal among farmers and laborers as well as seasoned politicians and members of the business and in-

dustrial elite? Also, why did this lifelong Republican, who was a major contender for the party's presidential nomination in 1888, abandon the GOP in 1892, toy with the idea of running on the Populist ticket, and ultimately endorse the Democrat Cleveland?

Calhoun partially explains the enigmatic aspects of Gresham's life by stressing that his traditionalism, characterized by a bedrock commitment to the civic virtues of republicanism, was the key to his popularity with the American public. In a period of dizzying economic change and social dislocation, Walter Q. Gresham was perceived as the Cato of his age, urging fellow citizens of the republic to return to the simpler, traditional values of an earlier revolutionary generation. When put to the test himself as a guardian of republican virtue, Gresham inspired public trust. Initially as a federal judge and then as postmaster general in the administration of Republican Chester A. Arthur, he proved to be an honest, efficient public servant dedicated to the public interest, a paragon of civic virtue. Gresham's decision to switch party allegiance reflected not only a personal animosity for Benjamin Harrison but growing disgust with party corruption and the widening disparities in wealth and class—he became the Jeremiah of the republic.

In assessing Judge Gresham's response to the issues of depression, labor unrest, the problem of overproduction, and impending revolution, Calhoun finds fault with the previous interpretations of Walter LaFeber and William Appleman Williams that depicted Gresham as an economic expansionist who sought foreign markets for the nation's agricultural and industrial surplus. In his view, Gresham favored neither political nor economic expansion abroad. Instead, by limiting foreign entanglements and reversing the prevailing expansionist agenda, he hoped to return the nation to the more traditional foreign policy of the founders. At Gresham's direction, the Cleveland administration withdrew Harrison's Hawaiian annexation treaty and pursued a non-entangling diplomacy in confronting the challenges to American interests inherent in British meddling in Nicaragua, revolution in Brazil, and the threat of war and partition in China.

Perhaps Calhoun too readily dismisses the insights of the "New Empire" proponents. Gresham offered solutions to existing problems that went beyond nostalgic yearnings for things as they were. His supporters understood his vision extended forward as well as backward. Part of that forward-looking vision was Gresham's awareness that foreign markets were needed to alleviate the nation's economic troubles. He chose tariff reduction as a means to that end, and he proudly related

that British ambassador Julian Pauncefoot understood such a change in America's economic policy would bring an "evil day" for Great Britain.

Finally, all may agree that Gresham was an anti-colonist, but it must be understood that he would countenance expansion if it were attained within the context of his principles. Adherence to republican values did not negate expansion per se. Gresham was more subtle than that; he favored expansion if the inhabitants approved and accepted incorporation in the American union. He said this outright concerning Canada and hinted that native Hawaiian approval for annexation would be a "quite different" question. But these are interpretive matters of honest disagreement that should not detract from Calhoun's achievement. He has enhanced our understanding of Walter Q. Gresham in his fine book.

EDWARD P. CRAPOL
College of William and Mary

W. ROSS YATES. *Joseph Wharton: Quaker Industrial Pioneer*. Bethlehem, Pa.: Lehigh University Press or Associated University Presses, Cranbury, N.J. 1987. Pp. 413. \$49.50.

Philadelphia industrialist Joseph Wharton is chiefly remembered for having endowed the first collegiate school of business in the United States, an enterprise whose centennial was recently marked with a fine institutional history (Steven Sass, *Pragmatic Imagination* [1982]). Now W. Ross Yates offers the first full-dress biography of Wharton since his death in 1909, a volume that sets his career in the context of the "industrial and financial practices" (p. 10) in family and corporate business, 1840–1900. Descended from a fairly prosperous clan of Hicksite Quakers, Wharton moved fluidly as a young man through networks of family and coreligionist partnerships before finding an outlet for his enormous (and discomfiting) ambition in metal refining, first zinc and nickel, later iron and steel making, and, in his last years, gold and silver ventures. Yates brilliantly evokes the maze of family obligations and contingencies of the market and technology through which Wharton picked his way to fortune, becoming by the 1880s one of that second echelon of industrial millionaires overshadowed by Andrew Carnegie and John D. Rockefeller, among others. At the same time, Yates is careful not to prettify his subject, for all of Wharton's brittle edges, sharp dealings, and corporate scheming are displayed unblinkingly. Like proprietary capitalists of his age, he labored to advance the interests of family and friends, was better disposed toward command than collaboration, and steeped himself in the

technical details of industrial processes, which are discussed with commendable clarity and economy by Yates. Extensive use of Wharton's papers and correspondence, preserved at Swarthmore College, brings Wharton to life, warts and all, a key element in effective biography.

As an innovator in the domestic production of zinc and nickel, Wharton scrambled to find outlets for his plants' capacity. Efforts to seduce the U.S. Mint into shifting its metal uses and to deflect foreign competitors embroiled Wharton in the politics of influence, as did later maneuvers on behalf of Bethlehem Iron's armor plate relations with the navy. Yates's accounts of the internal politics of Bethlehem are equally worth savoring, as are his sidelights on Frederick Taylor's famous tenure there. In profiling the Wharton School project, he adds little to Sass's work, but, in juxtaposing it to the metal magnate's quite different associations with Swarthmore, Yates heightens our awareness of the contradictions inherent in his subject's character.

There are, however, several shortcomings in this otherwise solid study. The book is innocent of contact with most of the last decade's research in business history, the history of technology, and Philadelphia studies. This isolates the work from significant debates swirling about the "modern business enterprise" (Alfred Chandler), metal-working and mass production (David Hounshell), and state-industry relations (Thomas McCraw among others). Yates does not attempt to measure Wharton's life against those of other "men in business," the captains and lieutenants of industry in the Gilded Age. Whereas business practices are richly developed, the reader learns little about Wharton's financial progress, returns on investments, and so on; information gleaned from the series of his business ledgers is presented anecdotally rather than analytically. Finally, a topical approach to Wharton's busiest years (1880–1900, chapters 9–17) yields confusing, overlapping chronologies that the reader must integrate to gain a sense of process. The book will be valued as a reservoir of material bearing on substantive questions about the transition from personalized to bureaucratic capitalism but not as an effort to frame and respond to them.

PHILIP SCRANTON
*Rutgers University,
Camden*

MICHAEL T. ISENBERG. *John L. Sullivan and His America*. (Sport and Society.) Urbana: University of Illinois Press. 1988. Pp. xii, 465. \$24.95.

By all accounts, John L. Sullivan was late nineteenth-century America's first and best-known

sports celebrity and arguably the best-known man in the country. "The Great John L." earned his reputation by taking on all comers in what passed for the white world of professional boxing, traveling throughout the United States and half-way around the world defending his heavyweight boxing championship until defeated by "Gentleman" Jim Corbett in 1892. Michael T. Isenberg's new biography is at its best in detailing the course of Sullivan's career as boxer, thespian, and celebrity. Less satisfying, though often intriguing, are his efforts to place Sullivan's story in the context of both his Irish working-class subculture and the larger society in which he lived.

As a hero of the Gilded Age, "the Boston Strong Boy" was known as much for his failures as for his success. The first sports figure to capitalize on and be manipulated by a national communications system, he lived his life in public view, one that Isenberg fully describes. We get not only a blow-by-blow description of every fight, a reckoning of his winnings (estimated at close to \$1 million), detailed accounts of his post-pugilistic careers as actor and temperance speaker but also a sense of Sullivan's darker side—his alcoholism, gambling, his losing battle to stay in shape, his raging temper, his mistreatment of women, his racism, and his public embarrassments in the ring and out. Although at times tedious, too long, and punched through with clichés, Isenberg ably traces the transformation of boxing from illegal bare-knuckle fighting on muddy fields and river barges controlled by working-class fancy into acceptable sport and business under formal rules in indoor arenas managed by middle-class entrepreneurs.

Isenberg's account parallels and at times departs from Elliott J. Gorn's *The Manly Art: Bare-Knuckle Prize Fighting in America* (1986). Both place boxing within a working-class bachelor subculture that intertwined male bonding, ethnic pride, alcoholism, violence, and rejection of the cult of domesticity in ways that produced, for Isenberg, a "cult of masculinity" and, for Gorn, an alternative definition of manhood to the one offered by middle-class America. For Gorn, much as Roy Rosenzweig and John Bodnar have suggested, this subculture provided a different source of values, self-esteem, and community. Isenberg demonstrates, however, that this male domain also spawned crime, violent alcoholics, and wife beaters. A conscious effort to integrate these images of working-class life with other studies of working-class culture and leisure would have been helpful.

More uneven are efforts to place Sullivan in the larger context of the Gilded Age. Isenberg tells us that Sullivan possessed a universal male appeal, one that hinged on his explosive power, which produced extraordinary victories symbolic of the

"vital energies of an expanding nation" (p. 379). As "America's first real life urban folk hero" (p. 380), he personified a traditional emphasis on individualism and strength but in a setting that underscored the emergence of a modern industrializing society. Several decades before Babe Ruth, the quintessential hero of excess, came on the scene, boxing promoters understood Sullivan's value in a developing consumer culture and made the most of it.

Although provocative, much of this analysis depends on a view of late nineteenth-century America that lacks full appreciation of the class conflict and anxiety that accompanied the emergence of a modern society. Noticeably absent is any serious consideration of those historians who have understood concern over such matters as a growing immigrant working class, the close of the frontier, and middle-class materialism as critical for understanding the rise of modern sport and the popularity of a Sullivan. In short, although it needed more direct engagement with the current work of American social and intellectual history, this book demonstrates the continued promise and potential of the new focus on sport as a significant and revealing part of American culture and society.

PETER LEVINE
Michigan State University

LOUIS GALAMBOS and JOSEPH PRATT. *The Rise of the Corporate Commonwealth: U.S. Business and Public Policy in the Twentieth Century*. New York: Basic. 1988. Pp. xiv, 286. \$19.95.

If Louis Galambos and Joseph Pratt intend this volume as a textbook, it should do well. They offer a lucid, concise, and well-reasoned account of government-business relations in the twentieth century. Unlike the usual bland fare of texts, this book has an interpretive overview that is both thoughtful and persuasive.

Using J. P. Morgan and Lee Iacocca as the symbolic bookends for the era under discussion, the authors trace the rise of business organizations through an evolving landscape of public policy. Internally, this involved the transformation of small entrepreneurial firms, which did well at innovation, into large corporations that lost much of their innovative ability in the drive to secure efficiency and control risks. Public regulation arose as part of the fallout from this process.

Externally, business evolved from a truly private sector to the modern corporate commonwealth of shared power. This transition is broken down into three phases. Between 1901 and 1939, business consolidated its control and endured its first major

crisis, the Great Depression. From 1940 to 1969, it soared to a dominant position at home and abroad. Since 1970, the corporate commonwealth has been shaken by its second major crisis, from which the recovery has already begun. This second upheaval involved a combination of difficulties at home and abroad familiar enough that they need not be itemized here.

Throughout this tale runs the theme that "[t]he corporate commonwealth, both its private and public wings, involved a series of compromises . . . between control, innovation, and efficiency" (p. 177). Both major crises forced shifts of emphasis. The urgent need for stability and security imposed by the Depression led to an enlarged role of government in business at the expense of innovation and efficiency of performance.

By World War II, this accommodation had been worked out well enough for business to exploit fully the advantages garnered it by the war and its aftermath. As corporations moved to a global scale, they learned of necessity to decentralize and diversify. This change, argue the authors, was as revolutionary as the first shift from the entrepreneurial firm to the centralized corporation. The American Era had arrived in all its glory, but its reign was surprisingly brief.

The second major crisis shook American confidence by laying bare a host of weaknesses in this system, the most obvious one being "the lack of integrative institutions that would enable the United States to recognize the interrelated nature of its problems and to implement intelligent, system-wide solutions" (p. 229). Or, put more bluntly, "Missing was any sense of orderly, directed change" (p. 253).

While the story itself is familiar, it is told in a fresh and compelling manner. Any knowledgeable student of business history will find interpretive bones to pick, which is as it should be. The use of Morgan and Iacocca as emblems of their age is a bit strained, and connections between themes are not always explored. For example, the Interstate Commerce Commission is aptly portrayed as failing badly at its task compared to some other regulatory agencies, but no explanation for the difference in performance is given. A discussion of environmental policy starts out well, then comes up lame at the end.

The only glaring fault of the book is an utter lack of documentation. Although the authors acknowledge leaving footprints on the shoulders of others, the giants and lesser mortals remain anonymous except for Alfred D. Chandler, Jr. The bibliography offered for each chapter is helpful but a poor substitute, whether intended for students, scholars, or casual readers. This is a pity, for in most cases the research is state of the art.

Scholars reading this book may feel like someone watching a montage of clips from famous films and trying to guess the titles as each clip whizzes by.

Nevertheless, even informed readers will find this book a useful and welcome compendium of the familiar wrapped in a rewarding interpretive framework. The charts and tables are relevant and supplement the text well. Anyone seeking a brief, incisive account of business and public policy in this century will be hard pressed to do better than this work.

MAURY KLEIN
University of Rhode Island

GWENDOLYN MINK. *Old Labor and New Immigrants in American Political Development: Union, Party, and State, 1875–1920*. Ithaca: Cornell University Press. 1986. Pp. 301.

The author's interest in American exceptionalism leads her to ask why an autonomous class-based labor party did not emerge in the United States as it did in various other developed countries. The existing array of explanations is analyzed and rejected as unsatisfactory, partly because they attempt to explain what did not happen rather than what did. Gwendolyn Mink sets herself the more positive task of analyzing why American trade unions, under the umbrella of the American Federation of Labor (AFL), aligned themselves with the Democratic party at the end of the nineteenth century. In addition, she considers how this association helped determine the political order emerging in the first two decades of this century. Her explanation rests on the necessity for unions of skilled workers to protect their economic interests against the reality or threat of cheap, unskilled foreign labor using new technology. The usual tensions between skilled and unskilled workers were exacerbated by ethnic and linguistic divisions that, the author argues, precluded the formation of inclusive industrial unions or the establishment of a class-based political party. The craft unions aligned with the Democratic party in 1896 to preserve their autonomy against attacks from above and below, from the state in alliance with the employers, and from the new immigrants from Europe. Immigration was thus the first and decisive factor in politicizing the trade union movement, since it could only be countered at the level of national politics. The opening to the left in the Democratic party proved unsuccessful electorally until 1912; in the meantime, the AFL associated with the National Civic Federation to protect itself against "the rule of unfriendly law" by demonstrating its moderation and responsibility.

Although Mink's rejection of existing explanations of exceptionalism is not always convincing, her focus on the reasons for the AFL making the choices it did is welcome and, on the whole, persuasive. Labor's experience of the split labor market does help to account for its rejection of class-based politics, and her emphasis on the politics of Chinese exclusion in California as a model for the policy to be adopted for European immigrants is reasonably convincing, although the analogy is pushed too far. In fact, the Chinese model misleads her into supposing that labor's opposition to both Asiatic and European immigrants was racist. Labor did not subscribe to the theory of racial determinism as far as Europeans were concerned, even though it believed that large numbers of new immigrants could not be assimilated. By contrast, labor was satisfied that the Chinese were racially immune to Americanization, whatever their numbers. Labor made comparisons between the two groups on the basis of behavior rather than race.

Mink's argument that the old unions made only token gestures to recruit unskilled European newcomers to the labor market overlooks the many genuine efforts to mobilize such workers, the success that was achieved both before and after 1900, and the strong sense of solidarity that persisted through the 1890s. There is also need for greater caution in assessing the degree of labor support for the Democratic party in 1896. Free silver was still an obstacle to many unions, the AFL did not formally endorse William Jennings Bryan, and the many defections from the Democrats in the industrial Northeast in 1896 are not necessarily explained solely by the growing allegiance of nonunionized industrial workers to William McKinley. Finally, Mink's own evidence suggests that the unions' growing rapprochement with the Democrats arose as much from the use of the law in industrial disputes as from the saliency of immigration. Mink's thesis is wounded by these criticisms but not mortally; it remains a valuable contribution to the debate over American labor's exceptionalism.

THOMAS LANE
University of Bradford

DAVID MONTGOMERY. *The Fall of the House of Labor: The Workplace, the State, and American Labor Activism, 1865–1925*. New York: Cambridge University Press or Editions de la Maison des Sciences de l'Homme, Paris. 1987. Pp. xii, 494. \$27.95.

Perhaps more appropriately labeled "Inside the House of Labor," David Montgomery's book is a splendid, unparalleled exploration of the many

mansions of the American worker's world from the 1880s through the 1920s. It is also a most complicated work, combining several discrete research essays on the social history of the workplace with a narrative of labor-management-state conflict. Drawing on the author's own unsurpassed research on the American work experience and including the best synthesis to date of a wide-ranging secondary literature, the book provides a feast for labor historians and a volume of near-encyclopedic reference for other students of American history.

Among Montgomery's several major themes, the first might be labeled the variety of American working-class experience. With exquisite detail he reconstructs the distinct worlds of the skilled male worker (particularly, iron and steel workers and machinists), the immigrant and Afro-American male laborer, and the female factory operative of the garment and textile trades. Montgomery's treatment of laborers is particularly noteworthy for its integration of world systems theory and his consequent ability to link the experiences and responses of those drawn off the land of the capitalist periphery into the industrial center: "The laborer from Italy's *Mezzogiorno* seldom said '*Lavaro*' ('I am working'). He said '*Sto fatigando*' ('I am expending myself'). Said Slovenes, '*Pomali delati, pomali krasti*' ('Work a little, steal a little')" (p. 92).

The book's midsection masterfully sets the stage for the bitter industrial relations of the period. If the deflationary crisis of the 1870s first stimulated the drive toward mass organization among American workers, then the deeper depression of the 1890s, Montgomery argues, touched off initiatives among employers that defined the battleground for twentieth-century labor struggles. In his three middle chapters, Montgomery treats the revolution of "scientific management" in the metalworking industries, shedding new light on both business organization and shop-floor response in the prewar years. In doing so, he convincingly denigrates as merely "tactical" (p. 273) the sociological distinction commonly drawn between the corporatist National Civic Federation (NCF) and the virulently antiunion National Association of Manufacturers. To take another example, while usefully emphasizing an agreement between metal trades employers and union leaders in 1900 as a model of "professional" settlement of disputes, Montgomery notes that the integration of business with labor leaders included telling social limits. While wives of capitalists "dutifully attended" the dinners and evening engagements planned by NCF leaders, "the wives of trade unionists were conspicuously absent" (p. 280).

The last three chapters of the book range from

the explosion of prewar militancy across industrial America to the search (by organized labor, middle-class reformers, and the wartime Wilson administration) for a new political structure for industrial relations and, finally, to the subsidence of labor protest in the 1920s. Without asserting any dramatic new paradigm, the author, nevertheless, fills in many holes of scholarly neglect. Through careful attention to such moments as the rejuvenation of the mineworkers' union in the 1897 bituminous strike, the role of young women operatives in the 1916 Turtle Creek Valley strike against Westinghouse Electric (where the twenty-one-year-old strike leader, Anne Katherine Bell, arrived at a union meeting with suitcase in hand after being thrown out of her house by her parents), the extraordinary career of Kansas City attorney Frank Walsh ("no other political figure of his time . . . so clearly personified the possibility . . . of a labor party" [p. 361]), and the ignominious expulsion of William F. Dunne for his left-wing political views from the 1923 American Federation of Labor convention, Montgomery offers up a fresh cast of decisive characters and events.

In the end, Montgomery's rich harvest of research accomplishes more than perhaps even he acknowledges. "The fall of the house of labor," with its implied emphasis on the effectiveness of scientific management and postwar drives for the open shop, really does not do justice to the extensive but heretofore neglected history of worker activism and labor resistance that he describes for the early twentieth century. Rather than settle for the conventional wisdom that American industry had been effectively pacified by the mid-1920s, Montgomery might more boldly have argued for continuity between the upheaval of the war period and that to come during the next depression. Certainly, those who would argue either side of that proposition—and any number of other questions—now have the benefit of a definitive text from which to start.

LEON FINK

University of North Carolina

JOHN MCCLELLAND, JR. *Wobbly War: The Centralia Story*. Foreword by RICHARD MAXWELL BROWN. Tacoma: Washington State Historical Society. 1987. Pp. xiii, 256. \$15.00.

John McClelland tells the powerful story of Centralia, 1919: the attack by American Legionnaires on the Industrial Workers of the World hall, the death of four Legionnaires, the lynching of IWW member Wesley Everest, and the subsequent conviction of eight Wobblies for second-degree mur-

der. Using oral history and manuscript collections, McClelland adds new details and nuances to the story. But his account moves within the framework of the earlier accounts published by the Legion, the IWW, the American Civil Liberties Union, and the Federal Council of Churches. Like these earlier versions, which were written while the Wobblies were on trial or in jail, *Wobbly War* focuses its analysis on the issue of guilt. Like the Federal Council of Churches in particular, McClelland faults the IWW for its plan of defense while placing primary responsibility for the tragedy on the Legionnaires and lumber barons (for vowing to attack the hall) and on Judge John Wilson (for preventing the defense from making its case and for imposing unduly harsh sentences). This focus on blame prevents McClelland from illuminating more general issues concerning the Wobblies, some of which were raised by Robert L. Tyler in *Rebels of the Woods* (1967).

With the help of numerous illustrations, McClelland tells his story well. He succeeds in recreating the atmosphere of anti-IWW hysteria in the Pacific Northwest during and after World War I and in showing how it shaped not only the plan to attack the hall but also the subsequent behavior of the lynching party, of Judge Wilson, of the jury. What he does not do is explain why loggers joined the persecuted IWW in the first place, or thought their hall worth defending against superior force, or declined to renounce the IWW even when by doing so they might have gotten out of prison long before 1933. Although he talked or corresponded with three of the defendants, McClelland did not succeed in eliciting from them an understanding of what they thought they were doing as Wobblies. The campaigners and lawyers for the defendants come alive in his account, but the loggers themselves remain shadowy figures. As community history, *Wobbly War* is exciting but incomplete.

McClelland—who grew up in the 1920s, thirty miles from Centralia—has grasped from within the fear and hatred of the IWW felt by conservative citizens. He has exposed the old anti-IWW myth, but he has not replaced that myth with a new portrait of the Wobblies.

STEVE GOLIN
Bloomfield College

PAUL F. CLARK *et al.*, editors. *Forging a Union of Steel: Philip Murray, SWOC, and the United Steelworkers*. Ithaca: ILR Press, for the New York State School of Industrial and Labor Relations, Cornell University. 1987. Pp. vii, 153. Cloth \$22.50, paper \$8.95.

A symposium was held at Pennsylvania State University on the fiftieth anniversary of the founding

of the United Steelworkers of America (USWA) and the one-hundredth anniversary of the birth of Philip Murray. Its purpose was to stimulate further study of the widely acknowledged role of both the man and the organization in the development of the modern American labor movement. Perplexed by the scholarly neglect of Murray and the USWA, symposium organizers commissioned four influential scholars to identify and discuss major themes and specific issues that other historians interested in the contributions of steel unionism might pursue in their own work. To sharpen further the resulting dialogue, union activists and labor journalists familiar with the early years of steel unionism, I. W. Abel and Abe Raskin among others, were invited to participate in the symposium and to provide their own unique perspectives on the subject.

This small volume represents a result of that effort. David Brody examines the origins of modern steel unionism during the era of the Steel Workers' Organizing Committee (SWOC). He argues that, unlike other industries, most notably the auto industry, rank-and-file pressures did not have a strong influence in shaping the union movement that emerged in the steel industry. Because of the prior existence of the Amalgamated Association of Iron, Steel, and Tin Workers and the company-dominated Employee Representation Plans (ERPs), a labor-management institutional structure already existed in the industry. Union organizers in steel, then, were more influenced by the nature of SWOC, the organizing instrument, and by the faith and character of Murray than by worker militancy. Rather than mass uprisings of steel workers, Murray and the SWOC used staged press conferences, political attachments to the New Deal, capture of the ERPs, and government intervention to organize the industry.

After a brief comparison of the differing personalities and temperaments of John L. Lewis and Murray, Melvyn Dubofsky, in an essay entitled "Labor's Odd Couple," proposes to examine how and why they "came to a parting of the ways" (p. 32). Much of the essay, however, is devoted to the period during which the two men worked together harmoniously. The analysis of their estrangement includes a discussion of the two men's well-known political differences, especially regarding Franklin Roosevelt, and Lewis's compulsion to dominate and control everything and everyone within his grasp.

In a substantial essay, Mark McCulloch identifies several themes and issues that could be investigated by scholars interested in Murray and the USWA. McCulloch's essay covers the period 1937–42, and he begins with the observation that schol-

ars have largely ignored "Little Steel" after the dramatic 1937 strike. He also calls for studies of the organization of "small steel," of white-collar workers in the industry, of the steel strike of 1946, and of two basic steel companies—Armco and Weirton—that were not organized. Along somewhat different lines, McCollock cites the need to know more about the composition of the work force in steel, particularly the participation of women and blacks. Other areas in need of study include wage structure and the steelworker's standard of living, the shop floor and grievance procedures, union governance, and political affairs.

In the final essay Ronald Schatz explores the relationship between labor and government during the 1930s and 1940s that did so much to redefine labor-management relations. Murray, Schatz argues, nurtured a union-government alliance that facilitated the unionization of basic industries. As a consequence, in the postwar years the steel industry ceased using company police and scabs to break the union and instead employed lawyers and public relations firms to attack the political linkage between USWA headquarters and the White House. Disruption of a labor-government alliance has become, of course, all too familiar to observers of industrial relations during the 1980s.

GARY M. FINK
Georgia State University

JOYCE L. KORNBLUH. *A New Deal for Workers' Education: The Workers' Service Program, 1933–1942*. Urbana: University of Illinois Press. 1987. Pp. xii, 175. \$24.95.

Joyce L. Kornbluh has written a valuable monograph on an important subject, the workers' education program of the New Deal. Her book traces the concept of workers' education to the Progressive era and the particular form workers' education took in the 1930s to the ideas of Hilda Worthington Smith, a wealthy, well-educated, liberal patrician who grew up on an estate across the Hudson River from Hyde Park. As a dean at Bryn Mawr College in 1921, Smith established the influential residential Summer School for Women Workers that ultimately received the combined support of the Women's Trade Union League, the needle trades' unions, the YWCA, and the federal and state departments of labor. Bryn Mawr's program was so successful that it became a model for the creation of other residential workers' education programs. By the later 1920s, Smith had emerged as a national leader of this ongoing progressive movement. As Kornbluh clearly demonstrates, Smith was thus a "natural" choice to

head up the workers' education project established first in 1933 as part of the Emergency Education Program of the Federal Emergency Relief Administration (FERA).

Kornbluh's history of Smith's effort to establish a permanent federal workers' education program highlights the difficulties that progressive reformers faced when they tried to change the system through both emergency and relief efforts. Under the FERA and the Works Progress Administration (after 1935), federal workers' education programs were designed both to assist unemployed teachers and to educate workers about their economic problems, about federal programs, and about the labor movement. For a decade, federal workers' education efforts suffered because the tension between these two goals was never resolved. Moreover, at the local level, the content of New Deal workers' education programs seems to have varied considerably. Unfortunately, the "top-down" perspective that Kornbluh employs throughout her short book does not shed much light on these variations. As a result, the uninformed reader will be unable to judge whether or not the conservative congressional critics of New Deal workers' education who ultimately terminated the program were correct when they alleged it spread industrial unionism and left-wing political ideologies. Certainly, Kornbluh's description of the program's leadership in Washington reveals the absurdity of the charge that New Deal workers' education was "communistic." Yet, given the close connections between political radicals actually teaching in the program and CIO organizing efforts—Roy Reuther's activities in Flint from 1935 to 1937 are a prime example—I cannot help but suspect that the conservative attack on workers' education was based on a correct assessment of its political character at the local level.

This well-documented monograph raises many questions about the local impact of New Deal workers' education. It also prompts the reader to consider how New Deal workers' education fits into the larger "Americanization" effort that progressives have carried out among America's ethnic working people throughout this century. It is a credit to Kornbluh that her very professional book raises such serious questions for future study.

RONALD EDSFORTH
Skidmore College

SYDNEY STAHL WEINBERG. *The World of Our Mothers: The Lives of Jewish Immigrant Women*. Chapel Hill: University of North Carolina Press. 1988. Pp. xxiv, 325. \$22.95.

Sydney Stahl Weinberg has presented specialists in immigration, ethnic, and women's history, es-

pecially those who teach, with a useful book. As Abraham Cahan, editor of the *Jewish Daily Forward* at the turn of the century, discovered when he introduced a column called the "Bintel Brief" (bundle of letters), other people's personal lives, the basis for this book, make interesting reading.

Unlike Cahan's sources, however, the forty-six elderly Jewish women who provide the core of Weinberg's book (all immigrants from various places in the tsar's dominions) reveal more than their problems. They go beyond the dislocations and economic hardship that marked their lives to disclose lesser-known facts that the author has combined with information from a large variety of secondary works and marshaled into thirteen well-defined chapters.

The result is a book that enables the reader to view familiar facts from a new vantage point. It is well known, for example, that Orthodox Jews barred their daughters from highly valued religious study. What is less well known, however, is that this restriction prompted many East European Jewish women to become high achievers in secular schools (assuming that they got a chance to attend) and to pursue formal education all of their lives. Similarly, Weinberg's interviews suggest that the oldest daughter in a traditional Jewish family, usually seen as the most burdened, was also the most valued and, as a result, gained greater self-confidence than did her younger sisters who, in most cases, had an easier life.

Much of what Weinberg tells us is familiar but is interesting nonetheless, partly because those she interviewed, in spite of sharing a common religion and an Old World birth, led disparate lives. Time and place caused much of the difference; the women whose voices we hear in this book, for example, range in age from seventy to ninety-four and are thus from separate generations. In addition, the author chose the largest part of her sample to achieve balance, looking for differences in "social class, circumstances of emigration, age at the time of emigration, number of children in the family and birth order of the informant" (p. xxiii).

The results of her sampling methods are mixed. We learn that some of these women made happy marriages and some did not; some rebelled against the domination of a traditional father and others were docile; some went to school, others to work; and so on. This disparity is undoubtedly the reality, but it leads to problems. Because so many different experiences are recounted, generalizations are difficult to make. Apparently aware of this, Weinberg often uses the "on the one hand, on the other hand" construction, which is true to the data but unsatisfying to the reader, who is left with a cloudy picture.

When the women corroborate each other, as

they often do, there is more certainty, but the result is still not satisfactory. A repetitive pattern emerges that makes for monotonous reading. What Weinberg learned from these women should have been treated in the same way as the secondary works were, that is, studied and digested, not recited.

Readability, however desirable, is not the *sine qua non* of a scholarly book. The basic question is: does the reader close the book with a greater understanding of the world of East European Jewish women? The answer is yes, and for this reason the book, by personalizing some aspects of the immigrant experience, will be of considerable value to courses in immigration, ethnicity, and women's history.

SELMA BERROL
Baruch College,
City University of New York

ELISABETH ISRAELS PERRY, *Belle Moskowitz: Feminine Politics and the Exercise of Power in the Age of Alfred E. Smith*. New York: Oxford University Press. 1987. Pp. xv, 280. \$24.95.

Her grandfather urged, "When you grow up you must write a book about your Grandma Belle." Her father warned that it could not be done, since her grandmother "threw away her papers" (p. x). Elisabeth Israels Perry has responded to the urging and triumphed over the warning. This meticulously researched biography, a co-winner of the 1987 New York State Historical Association Manuscript Award, is not only a first-rate study of Al Smith's "Colonel House" but also provides a new prism for "feminine politics" in the Progressive era and in the 1920s.

Perry is particularly effective while tracking Belle Lindner in her early settlement and committee work, in delineating the making of a Progressive and in showing her role in forging the network of Jewish reformers and professionals that formed the backbone of New York liberalism through the New Deal. After her marriage to architect Charles H. Israels and the birth of three children, she continued her "good works" as a volunteer with the Council of Jewish Women and on various citizens' committees and earned money writing for *Charities* and *The Survey*. Perry sums up this early Progressivism: "Cleaning up dance halls was Belle Israels's way of expressing the reform impulse of her age" and her own philosophy, "Husband, then children, then house, and then career" (pp. 58, 60). With the congruence of personal tragedy in the death of her husband and the catastrophic Triangle Shirtwaist factory fire in 1911, "social service as a means of self-expression

gave way to social reform as a professional career" (p. 78). The young widow became a grievance clerk for the Dress and Waist Manufacturers Association, dealing, as at the settlement house, mainly with Jewish working women but this time confronting economic rather than moral issues.

After working for her causes in the Progressive party, she was introduced to the 1918 gubernatorial campaign of Alfred E. Smith by her husband of four years, Dr. Henry Moskowitz, and began the remarkable partnership that extended to her death in 1933. Her successful campaign strategy, urging Smith to take New York's newly enfranchised women seriously and her follow-up suggestion that the new administration set up an expert Reconstruction Commission (she served as executive secretary), resulted in recommendations that framed the agenda for Smith's four terms as governor and kept Progressivism alive in New York in its two major 1920s manifestations: social feminism and government reorganization. Linking Smith to her network of Jewish liberals and women, Moskowitz chose to serve without portfolio in the "kitchen cabinet." Quietly knitting in meetings, "speaking only when asked for her views," she "played the supportive wife who manipulates her husband's career from behind the scenes," and "knew her place and kept to it" (p. 153). Perry presents Moskowitz as a model of "feminine politics," eschewing position for influence and never threatening the establishment. (The height of her behind-the-scenes invisibility may have been achieved in the 1927 pre-campaign biography co-authored by Norman Hapgood and her husband, *Up from the City Streets*, in which she is completely absent from the index.) Within these parameters, Perry fully demonstrates the remarkable range of her responsibilities and the power of her influence.

Coordinating public relations and campaigns, the loyal Moskowitz was least effective in Smith's run for the presidency in 1928. Like Smith, she lost much of her effectiveness when she crossed the state line. Ironically, "The Selling of Al Smith," analyzing Moskowitz's role in Smith's presidential efforts in 1924 and 1928, is the least satisfying chapter. Perry, too, is not as sure-footed on territory beyond the Empire State. She has, however, in this fine personal and political portrait, broadened our understanding of the complexities of women and power in the first decades of American feminism.

DOROTHY M. BROWN
Georgetown University

ARLENE SCADRON, editor. *On Their Own: Widows and Widowhood in the American Southwest, 1848-1939*.

Urbana: University of Illinois Press. 1988. Pp. xx, 324. \$29.95.

In 1982, there were 12.6 million widowed people in the United States, almost 10.8 million of whom were female. The ratio of widowed women to widowed men, over the age of sixty-five, has grown from 2.4 to 1 in 1890 to almost 6 to 1 in 1986. Despite the prevalence of women among widows and the large proportion of women who, in the course of their lives, experience widowhood, historians of women in company with other historians have been remarkably silent on the subject. Arlene Scadron attributes our silence on widowhood to the dominance of men in the profession and to an aversion to women who live alone (a phenomenon to which witchcraft persecutions attest; see Marta Weigle's work on the subject as well as the copious literature of the Salem witch trials). But Scadron also blames Americans' discomfort in confronting death and dying. Attitudes toward widows have been inseparable from attitudes toward death, aging, impoverishment, and isolated adults, particularly in a society such as ours, which emphasizes the integrity of the nuclear family in its social customs and public policy. The history of widowhood has the potential to shed light on all these areas. This collection of eleven essays by scholars of history, literature, law, sociology, and anthropology, is seminal rather than definitive. It covers the era from the end of the Mexican War to the 1939 congressional amendment that expanded the Social Security Act's coverage of elderly wives, widows, and dependent children. The volume tantalizes the reader with its variety of methods and sources, regional comparisons and questions, and it points to the importance of further work on the subject.

The essays focus on Arizona, Colorado, New Mexico, and Utah. In them, there is no such thing as a "typical" widow. From the Hopi, who Alice Schlegel reveals have no word for "widow," to the widowed entrepreneurs of Arizona's Pioneer Society depicted by Deborah J. Baldwin, to the struggling unmarried Hispanic women of Santa Fe whom Deena González describes, the meaning and experience of widowhood is embedded in a temporal, cultural, political, economic, and social framework.

Although there are differences among Indians, Mormons, Hispanics, and Anglos as well as between them, there are also common patterns of coping. Despite the volume's title, widows were rarely on their own. Without slighting the trauma of their loss, the essays reveal that these women existed within a web of kinship or pseudo-kin networks that varied in their efficacy but were nonetheless there. Hispanic and Indian widows,

settled for generations in the area, adopted each other's children and called on cousins, uncles, brothers, and other kin for aid. Over time, these kinship networks were affected, often adversely, by the Anglo intrusion and the accompanying penetration of the cash-based capitalist economy Anglos dominated. On the other hand, as Scadron points out, Anglo women were at the start relatively isolated and rootless, having abandoned their kin back east. Nonetheless, these women could turn to the developing structure of Anglo society, for example, their husbands' business or labor associates, the local doctor, and female relief societies (these last particularly among the Mormons). For many, perhaps most women, widowhood meant constant struggle and desperation, but often such networks helped widows scrape together an existence, a little credit, food, or work from here and a little from there. Remarriage was a strategy common only among younger widows with young children, and, although the essays tend to see that pattern as resulting from male preference, it seems at least as likely a result of young women's greater need for help in supporting and retaining children.

In the fascinating final essay of the volume, Helen S. Carter examines legal differences in widows' status between the Spanish civil law tradition of Arizona and New Mexico and the Anglo-derived common law tradition of Colorado and Utah as the two traditions began to converge. It is the most thorough and lucid discussion of these property and inheritance laws and their implications available. Such legal differences, Carter makes clear, are not mere accidents of history but reveal differences in notions of marriage, community, kinship, property holding, and gender roles. This is one relationship between women and the state that should interest historians and political scientists desiring to bring the state back into historical analysis. Equally significant is the relationship of widows to public welfare policy. Throughout this era, public policy was aimed at children of widows, not widows themselves, reinforcing the invisibility of widows and ignoring their distinct needs. Institutional provisions reflected, as did the legal framework, dominant conceptions of women's proper place and helped shape the nature of widows' experience. It is telling that Maureen Ursenbach Beecher and her co-authors found that the typical female Mormon autobiographer ended the tale of her own life with her husband's death. The status and social and economic roles available to women helped determine to what degree widowhood marked a drastic departure in daily life. In most Indian societies—cooperative, communally oriented, and with a mixed economy—widowhood brought little dis-

ruption in status and security; in most Anglo societies—built around the nuclear family and female dependence on male earners—the reverse was true. We clearly need a sustained, coherent study that will explain why we have kept widows on the margins and pay closer attention to the role of widowhood in the life course of women, mutually to illuminate widowhood and American culture. This book is an important first step.

SARAH DEUTSCH
Clark University

JESSIE L. EMBRY. *Mormon Polygamous Families: Life in the Principle*. Foreword by LINDA KING NEWELL. (Publications in Mormon Studies, number 1.) Salt Lake City: University of Utah Press. 1987. Pp. xvii, 238.

Jessie L. Embry, director of the oral history program for the Charles Redd Center for Western Studies at Brigham Young University, has produced an important study that examines the operation of Mormon polygamy. Embry's study is based largely on information obtained from oral interviews conducted by the author and her assistants of some 250 individuals who during the late nineteenth and early twentieth centuries were the children of polygamous marriages. Embry has also utilized oral interview information gathered during the 1930s by Kimball Young and James Hullett, two pioneers in the field of Mormon polygamy. The resulting study is an enlightening, descriptive overview of this seemingly peculiar Mormon institution. But, as Embry quickly points out, the Mormons were not unique in their practice of polygamy, according to various anthropological studies indicating that the overwhelming majority of world societies (nearly 84 percent) have, in the past, embraced plural marriage. Embry herself effectively compares and contrasts Mormon polygamy with plural marriage as practiced in other societies and cultures, both past and present.

Embry is also effective in describing the actual day-to-day operation of Mormon polygamy. She articulates the varied motivations of why Latter-day Saints entered into and practiced polygamy, concluding that "the desire to live their religion" was the primary motivation (p. 52). In outlining the demographic characteristics of Mormon polygamous families, Embry notes that the "typical" polygamous male had fewer wives than might be supposed—60 percent had just two wives; 20 percent had three wives; 10 percent had four wives; and just 10 percent had five or more wives. This made Brigham Young with his vast family of fifty-five wives a conspicuous exception rather

than the rule. Most polygamous husbands, moreover, placed their plural wives in separate households and generally lived with each on a systematic rotating basis. The author also discusses daily life and family roles, changing relationships between the husbands and their wives, the varied relationships between wives within the same family, and the situation of children within plural families. She even broaches the controversial subjects of divorce and inheritance within polygamous families.

Despite a willingness to touch on certain controversial topics, Embry generally presents an extremely positive picture of Mormon polygamy. Justifying her approach, she notes that, while earlier studies "have emphasized the negative aspects of Mormon polygamous life, pointing to jealousies between husbands and wives, and between the wives themselves, husbands being absent and wives having to be self-sufficient, and families being divided, this study shows that while these experiences did occur, they were the exception rather than the norm" (p. 194). However, Embry goes too far the other way, coming across as overly idealistic and too apologetic. Also, Embry's organization and prose style are less than adequate. This work lacks a central theme, outside of its basic intent to refute earlier, more critical studies of Mormon polygamy. Its style of presentation is awkward and repetitive, often bordering on the pedantic. This is particularly evident in its basic chapter organization whereby the author tells the reader at the beginning what the chapter is going to be about, then elaborates on this material in the main part of the chapter, and finally summarizes what she has already described two times previously! Despite its organizational and literary problems, *Mormon Polygamous Families* is a solid, valuable work. But scholars interested in this topic would do well to consult two earlier studies, namely, Lawrence Foster's *Religion and Sexuality: Three American Communal Experiences of the Nineteenth Century* (1981) and Richard S. Van Wagoner's *Mormon Polygamy: A History* (1986).

NEWELL G. BRINGHURST
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GREGORY ORFALEA. *Before the Flames: A Quest for the History of Arab Americans*. Austin: University of Texas Press. 1988. Pp. xiii, 354. \$22.50.

This book is an exciting journey back to Gregory Orfalea's Arab roots and a discovery of his community both in the Arab homeland and in the United States. In the process, Orfalea provides a vivid picture of the "awakening" of Americans of

Arab descent to the fact that they are an ethnic community, namely, Arab-American. The blurb on the dust jacket accurately describes the book as in the same genre as Alex Haley's *Roots* and Irving Howe's *World of Our Fathers*. It is essentially a personalized account of the Arab-American community, its aspirations and challenges, as well as its interaction with the original homeland. While the narrative proceeds in a generally chronological sequence, the author cleverly intertwines the experiences and life stories of various community members, both famous and ordinary, so that the account flows smoothly and captures and retains our interest. The book is enhanced by a large number (thirty-eight pages) of photographs representing different facets of the life of the Arab community in the United States. Also, a glossary of Arabic words and a bibliography are very helpful to nonspecialists, particularly those who do not read Arabic.

Several themes are developed in this excellent study. One relates to the dismemberment of Palestine and the emergence of the Palestine question. To his credit, Orfalea treats this issue not merely as a "problem"; rather, he discusses intelligently the various facets of this tragedy and how it has affected the Palestinian Arabs, especially those who found their way to the United States as immigrants. Furthermore, his discussion of other groups within the Arab-American community (Egyptians, Iraqis, Yemenis, Syrians) includes their assessment of, and reaction to, the Palestine question and the Palestinians. Another theme developed in the book relates to Lebanon and its people before the civil war and the 1982 Israeli invasion (the "flames" of the title). This emphasis is proper since the author is part Lebanese and since most of the early Arab immigrants to the United States came from Lebanon.

While Orfalea traces the history and background of his community based on available sources and provides statistical tables in four indexes, his main contribution, and it is a major and unique contribution, is the intelligent interweaving of the author's background, identity formation, challenges and opportunities with the same issues facing the Arab-American community as a whole. In particular, Orfalea's oral history interviews with over 125 Arab-Americans are presented in a focused, perceptive, and informative manner. In their diversity, both in terms of the different regions in the United States as well as the individuals selected, these interviews give us a true picture and a microcosm of the Arab-American community.

Thus Ameen Haddad, Mama Ayesha, Assad Roum, Ahmed Shaibi, no less than the better-known celebrities such as Danny Thomas, Najeeb

Halaby, Michael DeBakey, Helen Thomas, Vance Bourjaily, Casey Kasem, and Ralph Nader (who refused to be interviewed perhaps because of his concern over potential "problems" of identification as Arab-American)—all these figures provide information and insights about their lives and challenges as members of an Arab community of over two million spread out in all parts of the United States.

This book is a real treat. It is intelligently and sensitively written, and should be of interest to students of American history and American ethnic groups as well as to Arab-Americans.

MICHAEL W. SULEIMAN
Kansas State University

JOSEPH M. PAPO. *Sephardim in Twentieth Century America: In Search of Unity*. San Jose, Calif.: Pelé Yoetz or Judah L. Magnes Museum, Berkeley. 1987. Pp. xviii, 450. \$19.95.

Nearly all survey texts of the American Jewish experience devote an overwhelming amount of space to the Ashkenazic immigrants and their adjustments and contributions to the United States. This is as it should be, for, of the roughly 3.5 million Jews who have come to the United States, more than 95 percent have come out of the German-Yiddish milieu of Central and East Europe. When Sephardic newcomers and their institutions are mentioned, it is generally in a seventeenth- and eighteenth-century context, in the origins and early evolution of Jewish life in America, or as an elite group peering quizzically or disdainfully at German Jews or Yiddish-speaking "Slavic" Jews from tsarist Russia engulfing the Lower East Side of Manhattan Island.

Joseph M. Papo, following in the direction of Marc D. Angel and Joseph A. D. Sutton, calls attention to the fewer (around twenty-five thousand), but just as vibrant, Sephardic immigrants of the twentieth century who left Ottoman Turkey, the Balkans, and the Arab world and spoke Ladino (Judeo-Spanish), Greek, or Arabic. Like the Ashkenazic Jews from Austrian Galicia, Lithuania, Russian Poland, and the Ukraine, the Sephardim from Izmir, Salonika, Monastir, and Damascus confronted a new and bewildering American culture. Most of them settled in New York's burgeoning Jewish neighborhoods, and, like the Ashkenazim, they lamented the "Americanization" of their children when it interfered with family and traditional values and rites. Growing concern for Jewish welfare and survival in Europe and the Near East led both Sephardim and Ashkenazim to debate vigorously the efficacy of Theodor Herzl's Zionist plans. Because Palestine was then part of

the Ottoman empire and because so many Jews still lived under Turkish rule, many American Sephardim ignored or rejected Zionism in the early decades of the century. Turkey's subsequent loss of Palestine after World War I and the destruction of most of the Sephardic communities in Greece, Italy, and Yugoslavia during the Holocaust moved nearly all American Sephardim toward a fervent pro-Zionist posture.

Papo's knowledge of Ladino and his access to many of the defunct Ladino publications in the United States allow him to re-create the debates and conflicts within the disparate American Sephardic communities, for, as Ashkenazim had their many *landsmanshaften*, so Sephardim organized themselves in the United States along the lines of their cities-of-origin. A number of weekly Sephardic periodicals existed alongside the more numerous Yiddish-language ones. Four valuable appendixes list and offer detailed information on all Sephardic societies in the United States; a fifth appendix presents lengthy biographical sketches of the nineteen most important personalities among twentieth-century American Sephardim, including Moise Gadol, Henry Perahia, Nissim Ovdia, Joseph Gedalecia, and Nissim Behar. Like Angel and Sutton, Papo is, in the best sense, an amateur who has provided a future professional historian with the significant names, statistics, institutions, events, ideas, and artifacts out of which the story of all American Sephardim will be written.

IRVING KATZ
Indiana University

PAUL C. P. SIU. *The Chinese Laundryman: A Study of Social Isolation*. Edited by JOHN KUO WEI TCHEN. New York: New York University Press. 1987. Pp. xlii, 311. \$47.50.

Few American ethnic stereotypes have been more enduring than the solitary, subservient Chinese laundryman. Paul C. P. Siu, the son of a laundryman, left home in the 1930s to study sociology at the University of Chicago. There Ernest Burgess encouraged him to explore his father's world. Scholars of Chinese immigration have long turned to Siu's dissertation for insights, and we are all indebted to John Kuo Wei Tchen for overseeing the publication of this fine study. Tchen's introduction, which places the study in the context of Siu's own experiences, enhances the book's value.

Although Siu's dissertation was completed thirty-five years ago with data and observations collected in the late 1930s, the approach has a great deal in common with recent social history. In the preface, Daniel Walkowitz notes Siu's careful re-

construction of the laundryman's living and working conditions, and there is a wealth of detail on the ethnocentric subculture that the isolated laundry workers created. But Siu saw the work above all else as "a study of the mind of the Chinese laundryman" (p. 294). In this sense, too, the book is reminiscent of the best recent studies in social history, a fascinating evocation of an immigrant, petty entrepreneur's mentality.

Based on such valuable documents as letters and extensive interviews, as well as on Siu's own participant observations, much of the book is a blend of the laundrymen's own words with the sociologist's analyses of these and suggestions of how they constitute a distinctive world view. Quite apart from the fascinating reconstruction of obscure lives, the book is notable for two important conceptual innovations. Siu uses the term "immigrant economy" to describe the distinctive economic strategies that ethnic minorities employ in order to survive in the urban environment. The term "sojourner" Siu borrows from other scholars, but he refines it in order to capture the peculiar situation of the laundryman as an extreme case of the marginalized migrant. Isolated from mainstream society, the laundryman never really came to terms with it. Instead, he strenuously cultivated his ties with Chinatown, while his goal remained to return to China where he might enjoy wealth and status as a "Golden Mountain Man." All of the laundryman's essential characteristics, Siu argues, flowed from his situation as a sojourner.

This book is very much a product of the Chicago School of Sociology. Siu's methods and sources resemble those in William I. Thomas and Florian Znaniecki's *The Polish Peasant in Europe and America* (1918–20), Nels Anderson's *The Hobo* (1923), and other classics. The theme of personal and social "disorganization," expressed through gambling and sexual promiscuity, looms large. Siu's own evidence, however, suggests sources other than disorganization for such behavior. Gambling was clearly a case of transplanted folk culture adapted to urban exigencies, and the laundryman's patronage of prostitutes was largely a product of laws prohibiting the immigration of Chinese women.

Occupationally, socially, and residentially segregated, shut up in the back of a shop working twelve- and fourteen-hour days, separated for long periods of time and often for life from his family and native culture, and cut off from mainstream society by race prejudice and language barriers, the laundryman faced an isolated and bleak daily existence. The insulated subculture he created for himself among his countrymen was a

testament both to his own fortitude and to the power of racism to distort human relations.

JAMES R. BARRETT
University of Illinois

NANCY FONER, editor. *New Immigrants in New York*. New York: Columbia University Press. 1987. Pp. ix, 318. \$27.50.

Nancy Foner has brought together an excellent collection of essays on recent immigration into New York City. Written by specialists in history, sociology, anthropology, and labor economics, the articles provide a stimulating appraisal of the impact of immigrants on New York and that of New York on the immigrants. The groups discussed, chosen on the basis of population size and to reflect the ethnic diversity in the city, are the Dominicans, Haitians, Vincentians, Grenadians, Jamaicans, Koreans, Chinese, and Soviet Jews. In addition, there is an essay by Ellen Percy Kraly on immigration policy and migration patterns and an analysis by Adriana Marshall of the role of the immigrants within New York's economy.

Five of the nine contributors to this volume are anthropologists, and therefore they based their research mainly on interviews and participant observation. Although the contributors pay relatively little attention to the historical background of the groups, historians will still find much of value. Immigration historians will see many parallels between the recent immigrants and those of the nineteenth and early twentieth centuries and will be able to gain insights by observing groups going through the assimilation and adjustment process now.

The articles develop themes that immigration and ethnic historians have long researched and discussed. A study of Dominicans by Patricia Pessar, for example, provides information on their impact on New York's garment industry and the changing role of women within the family; Susan Stafford discusses the role of race and ethnicity in the country of origin (Haiti) and in the United States, a topic also analyzed in an article by Foner on the Jamaicans. Linda Basch notes, in a study of those migrating from Grenada and St. Vincent, the niche that voluntary associations have filled in shaping immigrant perceptions of the United States, aiding in adjustment to the new country and in maintaining ties to the homeland; and Bernard Wong, in a study of the Chinese, concentrates on economic adaptation, family and kinship networks, and political organization. These five anthropologists also touch on issues of intergroup and intragroup relations, religion, and ethnic politics.

Sociologist Illsoo Kim's essay on Koreans and historian Annelise Orleck's study of Russian Jews, both largely based on personal interviews and participant observation, also provide information pertinent to historians. Kim's article emphasizes the shopkeeper role the Koreans have played in the economic structure of New York and the importance of family ties in developing this economic position. Orleck's contribution adds a new element in discussing the immigrant communities by offering some historical background on Brighton Beach, the area in Brooklyn where Russian Jews have settled, and by analyzing generational factors in adjustment.

This anthology is certainly not the definitive work on the new immigrants, but it is a good beginning. By asking the appropriate questions, discussing issues significant to all students of immigration and ethnicity whatever the discipline, and developing the themes that will surely form the basis for much future research, this work has done a service to scholars researching the experiences of these groups as well as those of earlier immigrants.

The book could have been improved by providing more of a historical context for the groups under study and adding other immigrants (particularly those from Colombia, India, Guyana, Ecuador, and the Philippines) who also make up important elements of New York's foreign-born community. These ethnic groups, however, will have to wait for the necessary research on their activities, and incisive historical analyses of all the new immigrants will have to wait until a historical perspective on their migration and adjustment is possible. Meanwhile, Foner's anthology offers a very good first look at New York's new immigrants.

RONALD H. BAYOR
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TINSLEY E. YARBROUGH, *A Passion for Justice: J. Waties Waring and Civil Rights*. New York: Oxford University Press, 1987. Pp. xii, 282. \$32.50.

J. Waties Waring (1880–1968) attained national renown as a federal district judge from 1942 to 1952. A lifelong resident of Charleston, South Carolina, and a member of its elite, Waring practiced law and allied politically with segregationist Democrats such as Burnett Maybank, Ellison D. "Cotton Ed" Smith, and James F. Byrnes. Though a typical white southerner in his first sixty years, as a federal judge Waring increasingly promoted civil rights as he ruled in a number of important civil rights cases.

In 1944 and 1945 he ruled that black and white

teachers should be paid equally, and in 1947 he decided that South Carolina's white primary was unconstitutional. Rejecting gradual racial reform, Waring became more outspoken, improved his personal relations with blacks, criticized southern moderates, and attracted publicity. His most dramatic break with the solid South came in 1951 in *Briggs v. Elliott*, a challenge to school segregation in South Carolina that became part of the 1954 Supreme Court case. Dissenting on a three-judge panel, Waring declared that segregation "is an evil that must be eradicated . . . Segregation is *per se* inequality" (p. 196). For his stands, Waring encountered great hostility and eventually retired to New York.

Tinsley E. Yarbrough, a political scientist, carefully and clearly tells Waring's story without special jargon or methodology. Devoting twenty pages to Waring's first sixty years and thirty pages to his retirement years, Yarbrough concentrates on the decade Waring served as a judge. The crucial question is why Waring's views changed from racially orthodox to radical. Yarbrough offers two explanations.

First, Yarbrough says Waring's second marriage had a profound effect. In 1945 he suddenly divorced his wife and married a twice-divorced Yankee. Shocked and scandalized, many Charlestonians ostracized the new couple. Critics charged that his second wife changed Waring's ideas. Yarbrough concedes that "the divorce was the initial catalyst for the Warings' isolation" (p. 41) from Charleston society and acknowledges that Waring's second wife introduced the judge to a wider range of ideas and people, but the author discounts the idea that she remolded Waring—"a strong-willed, dominating figure throughout his life" (p. 247).

Second, Yarbrough includes a quotation from Waring: "by being a federal judge I have gradually acquired a passion for justice" (p. 101). Though concluding that "the revolution in his thinking was sincere" (p. 248), Yarbrough provides in his rather dry account of court cases little analysis of that revolution. He also does not connect, or speculate about, the profound professional and personal changes that Waring underwent in his sixties.

The author, in addition, never sorts out Waring's breaches of judicial propriety in advising the NAACP (although Yarbrough is critical), Waring's peculiar relationships with his daughter and his nephew Thomas R. Waring, Jr., or Waring's and his wife's almost paranoid behavior in Charleston. Yarbrough also neglects other legal developments and the larger civil rights movement. He refers to few secondary works and instead depends heavily on Waring's personal papers and his oral history

memoir. Yarbrough does, nonetheless, effectively detail Waring's courageous work and resurrects his important story. Readers wishing a briefer introduction to Waring should read David Southern's fine essay in the *Journal of Negro History* (1981).

CHARLES W. EAGLES
University of Mississippi

ROBERT ERWIN JOHNSON. *Guardians of the Sea: History of the United States Coast Guard, 1915 to the Present*. Annapolis, Md.: Naval Institute Press. 1987. Pp. x, 412. \$23.95.

In his most recent book, the prominent naval and maritime historian Robert Erwin Johnson turns his attention to the U.S. Coast Guard. Johnson served as an officer in that service during World War II, and he concludes that the overall history of his former service is meritorious. Nevertheless, Johnson is a critical scholar who identifies the shortcomings of the Coast Guard, as well as its undoubted contributions to the nation.

The origins of this organization date from 1790, when the new federal government established a force of cutters to assist the Treasury Department in collecting import duties. In 1915, the Revenue-Cutter Service merged with the U.S. Lifesaving Service, another Treasury Department organization, to form the U.S. Coast Guard. Johnson primarily focuses on the history of the Coast Guard from 1915 through 1967. In addition to discussing its operations and institutional development, his comprehensive account provides insight into the Coast Guard's social history, including the roles of blacks and women. The development of Coast Guard ships, aircraft, and other technological subjects also receives welcome attention.

The expansion of the Coast Guard's responsibilities and the institution's notable versatility are among the major themes in Johnson's book. The Treasury's revenue cutters served as armed vessels in most of the nation's early wars. The Coast Guard's organic act of 1915 gave legal sanction to its role as a stand-by military force and assured that these operations would continue. Johnson provides full descriptions of the Coast Guard's experience in both world wars and in the Korean and Vietnam conflicts. The Coast Guard's traditional anti-smuggling mission also took on new dimensions when it sought to halt maritime rum-running in the 1920s and, in more recent years, to interdict the illicit importation of drugs. The organization's longstanding concern with safety at sea, resulting in many dramatic rescue operations, also broadened when it began to track the path of North Atlantic icebergs, establish mid-ocean

weather stations, and maintain Loran navigational stations throughout much of the world. In addition, Johnson recounts the Coast Guard's growing role in regulating and inspecting the American merchant marine, its absorption in 1939 of the U.S. Lighthouse Service, and the enforcement of modern fishing and ecological regulations.

While describing this growth in the Coast Guard's mission, Johnson also depicts the problems of a relatively small governmental agency that sometimes struggled for its existence. Many Coast Guardsmen complained that the Treasury Department, to which they were assigned until 1967, showed little interest in their programs. As a result, there were occasional efforts by Coast Guard officers to merge their service with the Navy. But this centrifugal force was offset by the determination of Coast Guard commandants to maintain and develop a distinctive maritime institution. Some of the most illuminating sections in this book describe the bureaucratic skill of Admirals Frederick C. Billard, Russell R. Waesche, and other notable leaders in building support for their organization in the executive and legislative branches. To a considerable degree, they achieved this goal by embracing new or expanded missions demanded by the nation's politicians.

Johnson's book is based on extensive use of Coast Guard records, the personal papers of key individuals, and major official and secondary works. It is a valuable addition to the literature on America's sea services.

DEAN C. ALLARD
U.S. Naval Historical Center

JOHN WHITECLAY CHAMBERS II. *To Raise an Army: The Draft Comes to Modern America*. New York: Free Press of Macmillan. 1987. Pp. xi, 386. \$24.95.

This is a valuable but uneven book. It is not a comprehensive history of conscription in the United States. The first quarter of the book treats pre-twentieth-century practices of military recruitment, starting with colonial militia and going through the infamous Civil War draft. This section makes a valuable contribution by summarizing and analyzing a complicated subject in a brief, clear fashion and by offering shrewd evaluations of earlier American experiences in raising armies, including a reconsideration of the role of conscription in raising the Union and Confederate armies. By contrast, less than a tenth of the book examines the draft and military recruitment since 1920; the treatment of these last seven decades is perfunctory. In short, this is a book about the draft during World War I, and it should be judged as a treatment of that subject.

The World War I draft has long needed a history, and *To Raise an Army* fills that gap admirably in two respects. The story breaks down into three parts: the debate over the draft before American intervention in the war, the implementation of conscription during American belligerency, and the dismantling of the "selective service" system soon after the end of hostilities. John Whiteclay Chambers covers the second and third parts of the story well. He describes and interprets the largely successful use of the draft to gather nearly three million men into the army (no other services required draftees during World War I) within a period of eighteen months. Chambers scants neither critics and opponents nor "dodgers" and exemptees, but he properly devotes greatest attention to the actual workings of the system. Likewise, he neatly recounts the wrangling in 1919 and 1920 that led to abandonment of all plans for possible future conscription.

Unfortunately, the first part of the story—the pre-1917 debate over conscription—receives much less adequate treatment. The most striking aspect of the adoption of the draft was its almost overnight transformation from the most controversial of political issues into a measure passed in both houses of Congress with surprising ease and speed. Before 1917, Theodore Roosevelt was the only major political figure who was willing to handle this hot potato. After the declaration of war, however, opposition to the draft virtually collapsed. Even some pacifists supported it, while black leaders clamored to have members of their race *included* under it. Clearly, the draft in 1917 lived up to the old saw about "an idea whose time has come." But why that was the case receives little illumination in these pages.

Chambers discusses pre-intervention draft agitation as part of the larger drive for military "preparedness," and he finds that support for conscription sprang from "members of the corporate-oriented business and professional elite" (p. 80). Perhaps so, but that explains almost nothing. As Chambers also observes, the anti-preparedness leadership fitted a similar social profile. He does not say much about the ideas that conscription advocates espoused. Readers who want to know more about the ideas behind the preparedness drive in general will learn more from the works of John Garry Clifford, John P. Finnegan, and Michael Pearlman. The values and aims that lay behind the specific advocacy of conscription get scant airing in this book.

Further, the exact way in which "selective service" swiftly displaced the conscriptionists' earlier calls for universal service needs better explication. The main movers in that shift were the professional military leadership, Secretary of War New-

ton D. Baker, and President Woodrow Wilson. Baker comes under fairly close study, as does Wilson. The generals involved, however, remain shadowy figures. Chambers also argues that the timing of Wilson's sponsorship of draft legislation owed most to his desire to smother Roosevelt's scheme to lead a division of volunteers to fight on the Western Front. He presents interesting circumstantial evidence for such a motive behind Wilson's speedy introduction of a conscription bill, and I am one of the last people who should discount the personal rivalry between those two men. Yet this argument strikes me as no more than an interesting speculation, not the proven case that Chambers maintains he has established. These shortcomings keep *To Raise an Army* from being an entirely satisfactory treatment of the World War I draft, but on the whole it is a valuable book about an important subject.

JOHN MILTON COOPER, JR.
University of Wisconsin

PAUL F. BRAIM. *The Test of Battle: The American Expeditionary Forces in the Meuse-Argonne Campaign*. Newark: University of Delaware Press or Associated University Presses, Cranbury, N.J. 1987. Pp. 229. \$34.50.

Like its subject—the American Expeditionary Forces (AEF), 1917–18—Paul F. Braim's book is long on promise and short on performance. The subject is important to understanding the closing days of World War I and the military background to the peacemaking that followed, but this work, a revised dissertation on the Meuse-Argonne campaign, is not the definitive study of the AEF's only major offensive campaign.

Until World War II, the Meuse-Argonne campaign was the largest and longest battle in which American troops had participated. Their performance demonstrated the strengths and weaknesses of American military culture, reflected in the AEF's unsubtle drive north as the eastern arm of the Allied "compressing envelopment" (Douglas Haig's term) that drove the Imperial German army back to the borders of the Reich. Most of the scholarly writing on the campaign (like my own) is top down, the battle as seen by the principal commanders of the AEF. Personal reminiscences and accounts of small units—literature on the "face of war"—compose the rest of the works available. Documentary evidence on the battle, however, can sustain more detailed analysis than either Braim or anyone else has yet provided.

The author has a simple thesis that is supportable: the AEF's battlefield performance did not convince the Germans or the Allies that the Amer-

icans could conduct major offensive operations. Braim agrees with the assessments of most current AEF scholars, including the late Donald Smythe, Pershing's definitive biographer. Braim, in fact, cites all of the current authorities frequently and extensively, which is a clear indication of his own intellectual insecurity.

Braim's uncertain grasp of his subject is easy to discover. He did not do enough research, nor does he show how much research he did. One cue is that only two chapters of eight in a short book actually deal with the campaign. The lack of research is particularly unfortunate because the author is a professional soldier of long service who could have brought a keen eye to his study. For example, he does not exploit the published documents in the U.S. Army's own documentary collection of AEF records or the even more extensive printed records of the First and Second Divisions. Although he cites the AEF records in Record Group 120 in the National Archives, the author does not show much use of this critical source of corps, division, and brigade messages and reports. Histories of divisions and small units abound in the library of the Army Military History Research Collection, and the Infantry School Library holds useful memoirs, some of which I used in writing about Robert L. Bullard's Third Corps. The French army's archives at Vincennes can provide detailed and learned reports from the French liaison officers with the AEF. Braim does use the collection of veterans' recollections at the Military History Institute, but a few quotes reminiscent of John Keegan are no substitute for a comprehensive, day-by-day reconstruction of the AEF's operations.

Test of Battle rests in that uncertain no-man's-land between books that educate the author and works that educate others. It is not a book that experts will find useful, and it is too short and superficial to introduce non-experts to the subject of AEF operations. Readers with a general interest in the topic should see Edward M. Coffman's *The War to End All Wars* and Smythe's *Pershing* for a point of departure. Although Braim's book could have been a truly important contribution to the literature on the AEF, it is stronger on assertions than on persuasive analysis.

ALLAN R. MILLETT
Ohio State University

JOHN F. SHORTAL. *Forged by Fire: General Robert L. Eichelberger and the Pacific War*. (American Military History.) Columbia: University of South Carolina Press. 1987. Pp. xi, 154. \$24.95.

The war waged by American and Australian forces in New Guinea and the Philippines during

World War II is probably among the least understood and remembered of the great land campaigns of that war and General Robert Eichelberger probably among the least-known generals. He has certainly received far less attention than his near contemporaries Dwight D. Eisenhower, George S. Patton, and Omar Bradley. This first book-length study of Eichelberger, one of General Douglas MacArthur's ablest commanders, might well have been subtitled "General Eichelberger and the Art of Leadership." John F. Shortal, in fact, pays relatively little attention to the relationship of Eichelberger's campaigns to the Pacific war as a whole and a great deal of attention to Eichelberger's methods and achievements as a battlefield commander.

Shortal makes a persuasive case that Eichelberger was "the most successful general in the U.S. Army [whose] troops would capture more enemy held territory, in less time than any other general of World War II" (p. 93). And, it might be added, conduct more successful amphibious landings than any Marine general.

If that is so, why is Eichelberger not better known? In part because of the obscurity of the war in the southwest Pacific. In part also because some of Eichelberger's most brilliant achievements have been credited to MacArthur. One of the great strengths of Shortal's book is to make clear the very serious shortcomings of "the American Caesar" as a theater commander and to show how many times he was rescued from near disaster by Eichelberger's adroitness at pulling his chestnuts out of the fire.

Shortal's demonstration of the way in which many of the most costly battles and setbacks of the New Guinea campaigns resulted from mistakes, misperceptions, or sloppy homework by MacArthur's staff is devastating and largely accurate. Occasionally, however, the author's obvious sympathy for Eichelberger and his problems get the better of him as when, in discussing the Leyte campaign, he blames MacArthur's intelligence officer, General Charles Willoughby, for not anticipating that the Japanese would heavily reinforce Leyte with troops from Luzon. Willoughby would appear on few lists of military geniuses, but his mistake in this case was certainly understandable. As the author himself acknowledges, the Japanese did, in fact, originally intend to keep most of their forces on Luzon and only changed their minds very late in the game.

Overall, this clearly written, well-organized book makes a strong case for including Eichelberger's name high on the list of great commanders of World War II.

RONALD H. SPECTOR
University of Alabama

ROSEMARY FOOT, *The Wrong War: American Policy and the Dimensions of the Korean Conflict, 1950–1953*. (Cornell Studies in Security Affairs.) Ithaca: Cornell University Press. 1985. Pp. 290. \$29.95.

War with China, Omar Bradley thought, would be "the wrong war, at the wrong place, in the wrong time, and with the wrong enemy." Rosemary Foot finds a title for her fine book in this oft-quoted remark but turns the tables on those who never cease repeating it in praise of the good sense of Harry Truman, the dangerous volatility of Douglas MacArthur, and the presumed lessons of Korea, "the limited war." In fact war with China was very close and narrowly avoided. Throughout the first two years of the Korean War, Truman and his advisers gave extended consideration to taking the war to China through air and naval power, walking a fine line between limiting and expanding the conflict.

The main focus of this book is on the policy debate over expanding the war, in three episodes: the full-blown crisis caused by China's intervention in Korea in late 1950, the ceasefire talks that began in July 1951 and the subsequent long delays that induced American frustration, and the changes brought about by a new administration in 1953. The book also dwells on changing perceptions of the Sino-Soviet enemy and how these influenced American global policy long after the war ended. The author's judicious reading of recent literature in political science on bureaucratic politics, perception and misperception, and so on provokes useful generalizations that will make the book interesting to political scientists as well as historians.

Retaliation against China was a regular part of American contingency planning from the time the war began; George Kennan was among the first to suggest "air and sea attacks" on targets in China (p. 83) should it enter the war (although he later became the lone significant critic of the American march to the Yalu). Later decisions of the National Security Council authorizing such retaliation under certain conditions undoubtedly convinced MacArthur that he had the authority to take the war to China (p. 85). Foot believes that Dwight D. Eisenhower was, nonetheless, more prepared than Truman to bombard targets in China and to use nuclear weapons. Yet evidence that has appeared since the book was completed makes even that distinction questionable: the Truman administration gave very active consideration to aerial and atomic attacks on China in the period December 1950–April 1951.

Foot shows that the Soviet Union acted with

restraint and caution from the day the war began, a conservative policy that ended up alienating North Korea and China for a long time to come and that must inevitably reflect back on the authorship of the initial invasion. China was left to defend North Korea and itself, as American forces seemed bent on marching up "the traditional Japanese invasion route into Manchuria, the industrial heartland of China" (p. 27).

The American interpretation of this evidence, ironically, was that the Soviets were rationally calculating their own interests and finding them based elsewhere than in the Far East, whereas a China willing to pour troops into Korea must be volatile, insurgent, ready to expand elsewhere. Thus, the author argues, the way was open to U.S. intervention in Indochina because the Russians would not care and the Chinese had to be stopped.

Foot produces unimpeachable evidence to back up her analysis—showing among other things that the American state is capable of keeping secrets, in this case ones that render obsolete much of the previous literature. The author's understated and concise style may lead some readers to miss just how extensive and wise is her use of new archival materials and just how much she does to demolish the received wisdom on the Korean War. This book will remain a standard in the literature for many years to come.

Ultimately, the war was a tragedy, if not a travesty. As Foot says, even in its "limited" form the war wrought mass devastation and millions of casualties, but it is all the more appalling when one understands that the war was "essentially a civil dispute in its origins" and "wholly unnecessary in terms of its provocation of the Chinese" (p. 37).

BRUCE CUMINGS
University of Chicago

KUROSS A. SAMII, *Involvement by Invitation: American Strategies of Containment in Iran*. University Park: Pennsylvania State University Press. 1987. Pp. xvi. 190. \$24.95.

This book is not the rigorous analysis of American strategies of containment suggested by its title. Rather, it is an extended essay on a number of subjects related to the involvement of the United States in Iranian affairs. Its subjects include the historiography and rhetoric of the cold war, Third World perceptions of Anglo-Soviet rivalry, the role of power in international relations (as well as the consequences of its possession), and a critique of liberal and conservative attitudes toward the question of social reform in the Third World. Kuross A. Samii writes gracefully, and he covers these subjects with insight.

Although Samii is concerned with broader issues of interpretation, he covers a lot of familiar ground, skipping from issue to issue and focusing first on one and then on another facet of American-Iranian relations. He has looked at documents from the Office of Strategic Services, the Joint Chiefs of Staff, the National Security Council, and the State Department, but his documentation is sometimes sketchy, and crucial sources are missing, particularly in his treatment of the tenure of Mohammad Mossadeq (see, by way of contrast, the documentation of Mark Gasiorowski's article, "The 1953 Coup d'Etat in Iran," *International Journal of Middle East Studies* 19 [1987]: 261–86). Many of the details and quotations in Samii's historical sketches, moreover, have been discussed and cited by others, and as a result there is little primary research that is new. What is new is a section of a chapter on propaganda in Iran during World War II (based on records from the Office of Strategic Services) and discussion (some of it possibly overstated) concerning U.S. military plans in Iran at the time of the overthrow of Mossadeq. Samii also reassesses the question of whether or not President Harry S. Truman was correct in his retrospective assertion that he sent an ultimatum to Joseph Stalin during the Iranian crisis, putting forward an interesting if unconvincing thesis that Truman did so through Walter Bedell Smith in April 1946. The conversation has been covered in greater detail by others writing on American-Iranian relations, and Samii gives it more weight than it deserves. But no one else so far as I know has suggested that conversation as the source of Truman's retrospective (and I believe erroneous) assertions.

In looking at the American-Iranian relationship and the transformation of the image of the United States in Iran, Samii sees 1954 as the key year. If Iranians supported efforts to invite the involvement of the United States in their affairs, their attitude shifted in 1954. He does not attribute this shift to the American role in the overthrow of Mossadeq, which was not widely known at the time; rather, he sees the key factor as the American and British takeover of the Iranian oil industry that followed in the wake of the coup.

Samii raises tough questions about postwar U.S. policies and offers some controversial observations about America's relationship with Iran and the Third World. If the opportunity permits, he argues, it is difficult for any nation, regardless of its values, to transcend self-interest in its foreign policies and act other than as an imperial power. Instead of disputing the patron-client relationship, he argues that imperial order imposes a responsibility on the patron to guide selected clients. In the case of Iran, the United States had

ample opportunity and leverage to compel the shah to clean up his act, but it failed to do so. Looking to the future, he believes that the destiny of the Third World will be determined by the race between reform and revolution and that, under the circumstances, the United States must compel its Third World allies to seek remedies for their appalling political and social inequities if it wishes to remain an imperial power and play that role with efficiency and grace.

BRUCE R. KUNIHOLM
Duke University

MARK HAMILTON LYTLE. *The Origins of the Iranian-American Alliance, 1941–1953*. New York: Holmes and Meier. 1987. Pp. xxi, 239. \$49.50.

Since the 1979 revolution in Iran, a spate of books on U.S.-Iranian relations has appeared. Only a few have deserved careful consideration, and Mark Hamilton Lytle's is one of them. The monograph presents a mildly revisionist interpretation, like the excellent dissertation from which it evolved. Lytle's revisionism sets his work apart from Bruce R. Kuniholm's *The Origins of the Cold War in the Near East* (1980), which in subject matter it most closely resembles.

With more understanding, patience, and good will from American diplomats, writes Lytle, Iran could have become an area of postwar cooperation rather than confrontation. Skeptics might conclude that this view reflects too much moral conscience, yet his arguments, which are supported by meticulous research in American and British archives and mastery of recent scholarship on the cold war, appear formidable. Forty years ago a well-known Iranian intellectual argued that Iran had to become the Switzerland of Asia; today, Lytle revives the not unreasonable proposition "that a neutral Iran would have better served the goal of a stable peace" (p. 217).

Lytle focuses on the period 1941–47, relating how policy makers step-by-step entangled the United States in Iran because they failed to question the assumption—which he considers unjustified—that the Soviets plotted expansion to the Persian Gulf. He points to shortcomings on both sides of the Iron Curtain, but the Soviet Union emerges more sinned against than sinning. In a single chapter Lytle covers the years 1948–53 and provides a good survey, but he underestimates somewhat Britain's importance. Lytle has done a fine job integrating recent scholarship with his conceptualization of the American-Iranian alliance. His organization of material indicates that he considers the earlier period more significant to the origins of the alliance, a conclusion some will question.

Commendably, Lytle uses scholarly works on Iran to bridge the traditional gap between diplomatic history and area study. He takes a refreshing look inside Iran, explaining why the Iranians initiated the alliance with the United States and how Prime Minister Ahmad Qavam saved Azarbaijan from slipping permanently into the Soviet orbit. (Surprisingly, the shah appears as only a shadowy presence here.)

Lytle rejects the simplism that the American-Iranian alliance can be understood solely in economic terms, yet he devotes pages to petroleum issues. No one denies their importance. But was the Azarbaijan crisis really about oil (p. 174)? A chapter on global oil provides a useful survey but interrupts what is otherwise a smooth narrative.

By contrast, a chapter on American advisers provides the keystone to Lytle's thesis. They came during World War II, ostensibly to stabilize Iran, but successive U.S. administrations denied them scarce resources and failed to resolve rivalries among them. Their trials and ultimate failures typified the difficulties of U.S. policy—then and later. At the root of the problem lay an American belief in exceptionalism, writes Lytle, a mistaken notion that Americans "would succeed in uplifting Iran where the old imperial powers like Great Britain and Russia had failed" (p. xix). We should recall, however, that Secretaries of State George Marshall and Dean Acheson had no illusions about uplifting Iran; in fact, they disparaged the likelihood of reform there, because they considered Iranians incapable of it. This attitude represented another variety of exceptionalism.

Lytle's book provides one of the best studies available on the subject. Lytle tells a complex story well, challenging some of our comfortable conclusions. He examines the problems of war and postwar adjustment from several perspectives, not just that of the United States. Although his study is not the final word, its depth of understanding and quality of analysis merit a wide audience.

JAMES GOODE

Grand Valley State University

TRUMBULL HIGGINS, *The Perfect Failure: Kennedy, Eisenhower, and the CIA at the Bay of Pigs*. New York: W. W. Norton. 1987. Pp. 224. \$17.95.

In late 1962 special assistant Arthur Schlesinger, Jr., sent a memorandum to President John F. Kennedy concerning Trumbull Higgins's offer to write a "serious and official history" of the Cuban crisis. The answer is not revealed by Higgins, but twenty-five years later he has written what is perhaps the most detailed account of the Bay of Pigs invasion.

Higgins begins with a chapter on the Guatemalan episode of 1954 that helped create the CIA's image of success conducting clandestine operations. This provided the backdrop for the overconfident mentality that underlay the planning, begun in 1960, to overthrow Fidel Castro. The operation plan rapidly grew from the infiltration of a few hundred guerrillas into the Escambray mountain chain to the landing of a conventional amphibious invasion force. This plan was the "hot potato," as Admiral Robert Dennison called it, that President Dwight Eisenhower handed to his successor.

Higgins does an excellent job of unraveling the twists and turns of the early months of 1961, as the president agonized over the confused details of the plan. The pessimistic evaluations by various military observers were sidetracked or buried. The complete details of the operations were not fully conveyed to the Joint Chiefs of Staff or to the navy. Even then Admiral Arleigh Burke felt that the CIA plan was "weak" and "sloppy" (p. 84). In the end, all concerned went along with the CIA's plan when confronted with warnings of the dire political consequences of abandoning the operation. The result was the failure of what Lyman Kirkpatrick (a CIA critic of the plan) called an operation "too big to be a raid and too small to be an invasion" (p. 166).

Higgins mentions the CIA's deliberate rejection of cooperation with Manuel Ray's leftist, anti-Castro movement. But he does not really discuss the CIA's refusal to supply arms to Ray's guerrillas in the Escambrays. Higgins's footnotes are a problem; they are difficult to use. When trying to pinpoint a source the reader must often wade through as many as twenty citations, without any guidance concerning the relationship of quotes and information to source. Higgins could use a good copy editor since some sentences are simply too long and turgid. And I am still not clear what is meant by referring to the CIA as a "fissiparous" agency (p. 38).

I cannot use the term "definitive" when dealing with such a topic. Not too long after this book's publication, Judith Campbell Exner revealed more of Kennedy's involvement with the Mafia in Castro assassination plots. In spite of vehement denials by protectors of the Kennedy flame, there are probably more unrevealed aspects of a Mafia connection. In the meantime Higgins's book will supply most of what one wants to know about this episode.

ROBERT FREEMAN SMITH
University of Toledo

MORRIS H. MORLEY. *Imperial State and Revolution: The United States and Cuba, 1952–1986*. New York: Cambridge University Press. 1987. Pp. ix, 571. Cloth \$59.50, paper \$16.95.

Morris H. Morley has written a most exhaustive study of the politics and economics of U.S. policy toward Cuba between 1952 and 1986. An extraordinarily detailed bibliography and footnotes, appendixes, and index attest to the range and thoroughness of his scholarship, based, as usual, on presidential libraries, the National Archives, various university libraries, and many interviews—often with unnamed persons.

What is most striking, however, in this study issued by a reputable university press is Morley's openly Marxist analysis of Cuban-American relations during the decisive years of the successful establishment of the first Communist state in the New World. Ignoring the United States' anxieties on this score, along with American military reactions to this issue as seemingly irrelevant, Morley indeed has a good case in describing the old Cuba of Fulgencio Batista as still essentially a U.S. sugar colony in a barely veiled form. What is controversial about this thesis, however, is Morley's apparent view that American resistance to the vast change resulting in Fidel Castro's anti-American as well as anti-capitalist country was motivated by essentially economic forces, although the documents that he cites *in extenso* reveal other American motives all along the line.

In fact the half-hearted efforts by American business through conservative U.S. ambassadors and the somewhat more liberal CIA under the Eisenhower administration to find some third force between Batista and Castro were invariably a failure; Castro thrived on his anti-Americanism and welcomed the cautious Soviet overtures as much as he was driven to them by U.S. actions. Morley is more revealing in his delineations of the sundry hypocrisies of the various Latin American governments toward the Cuban-American conflict. Torn by continuous, if sometimes ineffective, U.S. efforts to break the relations of the Organization of American States with Castro, the Latin American countries clearly did not wish to risk exacerbating popular sympathy for Castro at home and thus left the United States in its customary role as an aggressive Colossus of the North. Still, Morley does well to redress the balance of the usual American propaganda to the effect that its role south of the Rio Grande, like that in Europe, constituted no more than the advocacy of democratic political freedom against an alien Muscovite tyranny. On the other hand, even Kennedy liberals, though not unduly sympathetic with American

business at home, could hardly be expected to favor expropriation without compensation throughout Latin America. The more recent absurdities of the Ronald Reagan administration, of course, simply make duck soup for Morley's Marxist thesis, however much, in practice, Reagan's pseudo-surreptitious activities in Central America were impelled as much by the search for right-wing votes at home as by his intimate ties with American business.

TRUMBULL HIGGINS
John Jay College
City University of New York

WILLIAM C. BERMAN. *William Fulbright and the Vietnam War: The Dissent of a Political Realist*. Kent, Ohio: Kent State University Press. 1988. Pp. x, 235. \$24.00.

William C. Berman's book is one of those rare studies that is what it purports to be, the story of William Fulbright's dissent during the war with Vietnam—no more, no less. Berman is thorough and writes well enough, but the book is nonetheless unsatisfying. We are no closer to Fulbright than we were before. We never get inside his mind. Perhaps what we saw is all there was. Despite access to declassified materials and personal papers, despite interviews with Fulbright and his closest associates, there is little here unfamiliar to those of us who were following the activities of the senator during the war.

Berman describes Fulbright as a "realist," stressing the senator's sense of the limits of American power. He compares Fulbright very deftly with Dean Rusk, contrasting Rusk's "universalism" with Fulbright's awareness of the need for priorities. Yet it is not easy to discern much difference between the two men prior to Fulbright's move into opposition to the war in 1965. Both were highly intelligent Rhodes scholars from the deep South. Both were categorized as "liberal internationalists," eager for the United States to accept the responsibilities of power after World War II and deeply committed to working with the United Nations in the 1940s, 1950s, and 1960s—when that organization was still amenable to American leadership. Both accepted the need to contain the Soviet Union and the People's Republic of China. Of the two, Rusk was probably perceived as the more sympathetic toward the aspirations of Asian and African peoples. Fulbright was viewed suspiciously by American friends of Israel because of his appreciation of Arab nationalism. But there is little to prepare us for the striking differences between their perceptions of America's role in

Vietnam after 1965, certainly not in their apparent agreement on steps taken through 1964.

The story told in Washington in the late 1960s was that Fulbright discovered in 1965 that the president had lied to him about events leading up to American intervention in the Dominican Republic and decided to have an independent look at Vietnam. His staff investigators persuaded him that the president had deceived him about Vietnam as well. Berman's sources have provided him with the same story.

The bitter division that developed between Fulbright and the administration in the mid-1960s may have been derived from the senator's anger at being deceived; it may have been the result of an intelligent man's assessment of the mounting evidence that something had gone awry with American policy. Or it may have been the influence of Carl Marcy or Seth Tillman. Certainly the devil theorists in the administration perceived horns on both Marcy and Tillman, and the role of these two men needs more explanation.

One idea that emerges from Berman's evidence is that Fulbright used committee hearings for his own education. When he developed reservations about a longstanding policy, such as the American effort to isolate the People's Republic of China, he brought in the nation's leading authorities to educate himself as well as the American public. After interrogating witnesses, weighing the evidence, the senator was prepared to come to his own conclusions rather than accept those offered to him by administration representatives—with all of the intellectual independence and arrogance such behavior implied.

Berman does a good job of portraying Fulbright as a loner, choosing his own course, frequently alienating other senate dissidents. In his last years in the Senate he was easy prey for Henry Kissinger, who knew how to play on his intellectual vanity. Out of touch with Democratic party leaders in Arkansas, he was unprepared for the primary challenge he faced in 1974 from Dale Bumpers, a highly popular young governor. Consequently, he ended his political career with a primary defeat, in which his dissent was less an issue than was his apparent lack of vigor.

There may be more to the story, but we will probably have to wait for Tillman's "official" biography. No one outside of his immediate family knew Fulbright better, and it is unlikely that anyone else understood his thinking on foreign policy as well. For now, Berman's study is eminently useful.

WARREN I. COHEN
Michigan State University

NANCY BOYD. *Emissaries: The Overseas Work of the American YWCA, 1895–1970*. New York: Woman's Press. 1986. Pp. xv, 337. \$18.95.

This is a beautifully produced, lavishly illustrated volume that offers the reader both the delights and the frustrations of leafing through an old photo album. The text divides the overseas work of the American Young Women's Christian Association (YWCA) into three chronological periods; following an introduction to each section that points to general changes in YWCA philosophy and methods, individual chapters focus on selected countries. Eight out of ten chapters also have a biographical focus that provides the reader with verbal snapshots of a handful of fascinating women among the more than eight hundred Advisory Secretaries who served in widely dispersed locations around the world.

China surfaces in all three sections, as it should, since nearly 30 percent of the total years of service by American Secretaries were spent in China. The chapters on China constitute the most significant contribution of this volume to the historical record. The example of China displays with admirable concreteness and specificity the evolution in YWCA work as the organization abandoned noblesse oblige benevolence for professionalized social work, implemented a less hierarchical group method approach to planning and decision making that allowed it to adapt its program to the needs generated by multiple revolutions in modernizing China, and substituted at least a limited religious and cultural relativism for its initial commitment to Protestant evangelization. This dynamic has an almost precise parallel in the foreign mission work of orthodox American Protestantism; indeed, the relationship of YWCA work to denominational mission efforts needs closer analysis than it receives here. (So, too, does the relation of overseas work to the YWCA's extensive domestic operation.)

Despite the compelling nature of the individual stories told here, collective or rather serial biography as a methodological strategy has serious limitations as an approach to the book's ostensible purpose, a sweeping survey of the YWCA's overseas operations in the twentieth century. Because the principle of selection seems to be whose character was most interesting and whose archival record was richest, the picture of the YWCA's global work that emerges is somewhat distorted. One chapter is devoted to the six months Celestine Smith spent in Nigeria in 1934–1935, and another describes Sue Stille's six-year career in Uganda in the 1950s; together, they create the impression that the American YWCA was extensively involved in Africa. Yet a rough calculation based on

data supplied in an appendix indicates that less than 2 percent of the total person-years of service were spent in Africa. Similarly, there are whole chapters on the Philippines and Korea although the allocation of personnel to those locations was minimal. The erroneous impression created by these choices is compounded by Nancy Boyd's failure, especially notable in the third section, to describe and analyze the dramatic shrinkage—as measured by personnel—in the overseas operations of the YWCA after 1950. Other omissions create even greater distortions. Work in the Middle East, almost 10 percent of the whole, is virtually ignored. The most serious oversight, however, is Boyd's failure to recognize the extent to which YWCA work was dominated by war relief efforts, particularly in Europe during and after World War I. Roughly 40 percent of all Advisory Secretaries on the roster from 1895 to 1970 saw foreign service only in war-torn Europe. At a conservative estimate, this represents at least 20 percent of all person-years of service overseas. Choosing not to explore the European component of overseas work seems to proceed from an ideological bias; Boyd's evident commitment to contemporary YWCA goals includes an endorsement of cultural pluralism that applauds movement toward a global partnership between First World and Third World women. This perspective makes it important to counter charges of cultural imperialism in YWCA work or at least to emphasize its diminution. Such an interpretive framework virtually precludes an analysis of American overseas work in Europe.

The virtue of Boyd's study is that it introduces to historians of women a major expression of religiously based internationalism on the part of American women in the twentieth century and raises, even when it does not adequately answer, insistent questions about the function, in the twentieth century, of a women's organization devoted to working with women on a global basis.

PATRICIA R. HILL
Wesleyan University

GEORGE MARSDEN. *Reforming Fundamentalism: Fuller Seminary and the New Evangelicalism*. Grand Rapids, Mich.: William B. Eerdmans. 1987. Pp. xiii, 319. \$19.95.

In an excellent earlier book, *Fundamentalism and American Culture: The Shaping of Twentieth-Century Evangelicalism, 1870–1925* (1980), George Marsden described the origins of American fundamentalism. This book, while formally a history of Fuller Theological Seminary, is a continuation of that story. A master of the theological and institu-

tional complexities within American evangelicalism, Marsden uses Fuller as “a window through which to focus my study of recent evangelicalism and fundamentalism” (p. vii).

With the exception of two brief chapters at the end (an Epilogue and a Sequel), this study is confined to the twenty years from the founding of Fuller in 1947 to a major shake-up that brought David Hubbard to the presidency in 1967. After that date, Fuller came to represent an evangelicalism so diverse, and so divorced from its historic roots, that, unfortunately, its history does not fall within Marsden's intended scope.

Marsden's narrative tells of “the renewal of America's nineteenth century evangelical heritage as it developed from a reform within fundamentalism into a separate movement” (p. 8). Many of the conservative Protestant scholars who left mainstream seminaries in the 1920s and 1930s found themselves uneasily aligned in the public mind and, in fact, with the anti-intellectual revivalism of Billy Sunday. Some were genteel and learned men; many were intellectually indebted to Princeton and J. Gresham Machen. They yearned to produce a conservative Biblical scholarship that would rival liberal scholarship. In radio evangelist Charles E. Fuller these intellectuals found a kindred pilgrim in the search for respectability and a fund raiser with the ability to establish an educational institution.

Fuller Theological Seminary was intended to be a collection of the best and the brightest from the evangelical world. The original faculty, assembled by Fuller and Harold John Ockenga, included Wilbur M. Smith, Everett F. Harrison, Carl F. H. Henry, and Harold Lindsell. An early addition was Edward J. Carnell, who succeeded Ockenga as president. This evangelical elite was commissioned to write books that would reclaim the intellectual heritage of conservative Protestantism.

A number of separate stories follow. One recounts a largely unsuccessful quest for respectability by sometimes pretentious men who craved approval and acceptance from the scholarly community they were attacking. A second tells of a split between evangelicals and fundamentalists brought on by lingering theological differences and by an increasing moderation among the intellectuals at Fuller. Finally, there is the story that ends Marsden's account in the late 1960s—the departure of the whole “right-wing of theological stalwarts” from the seminary and the appearance of clear theological changes at Fuller. At this point, reports Marsden, “the new evangelicalism was near its end as a unified and progressive enterprise” (p. 259).

Marsden writes on three rather distinct levels. He is perhaps most successful in clarifying the

issues that divided post-World War II fundamentalism and evangelicalism. Second, his history of Fuller Theological Seminary is a fine example of how to weave the history of an institution into its intellectual and cultural milieu. Third, Marsden explores the biographies of many of the most influential figures in the new evangelical movement. Although his sketches of Fuller, Ockenga, Carnell, and others are often deft, they are too frequently left unfinished as the author moves back to his broader objectives. In that respect, this book is much more a beginning of the study of the new evangelicalism than it is an end.

DAVID EDWIN HARRELL, JR.
University of Alabama,
Birmingham

THOMAS B. ALLEN. *Guardian of the Wild: The Story of the National Wildlife Federation, 1936-1986*. Bloomington: Indiana University Press, in association with the National Wildlife Federation. 1987. Pp. viii, 212. \$18.95.

The National Wildlife Federation is the largest conservation organization in the United States with between 4 and 6 million members. This is the history of its first fifty years.

It is possible for an institutional history to rise above a cataloguing of programs, publications, and officers. Susan Schrepfer's *The Fight to Save the Redwoods: A History of Environmental Reform, 1917-1978* (1983) leaps from the history of the Save-the-Redwoods League to important issues in scientific, social, and intellectual history. Her book is as much about the era as it is about the organization. But Thomas B. Allen's book fails to emerge from the trees of institutional politics and policy to see the forest in which they occurred. His disappointing bibliography, consisting of twelve books and articles, is the most obvious indication that Allen has little interest in the cultural context in which his story takes place. His text gives no sign that he is aware of even basic works, such as those of Schrepfer, Donald Worster, Stephen Fox, Roderick Nash, and Samuel Hays. It is especially hard to believe that Allen could ignore a book as directly relevant to his subject as John Rieger's *American Sportsmen and the Origins of Conservation* (1975).

Another problem with this book is the lack of documentation even for direct quotations. Granted, Allen is not a professional historian (he works for the National Geographic Society's book service), but Indiana University Press should have insisted on higher scholarly standards. Part of the deficiency may be explained by the fact that the press published this book "in association with" the

National Wildlife Federation, which also holds the copyright.

Adding to the difficulty of finding pattern and significance in *Guardian of the Wild* is the fact that each chapter is chronological; we return again and again to the 1930s to follow such federation activities as conservation, education, and lobbying. With the partial exception of the organization's founder, Jay N. "Ding" Darling, the personalities Allen sketches are as lifeless as the federation's annual wildlife stamps. The most valuable feature of the book is not in the text at all but in appendices 8 and 9, which contain legislation and litigation "timelines." They present useful surveys of the achievements of modern environmentalism that this book otherwise avoids discussing.

RODERICK NASH
University of California,
Santa Barbara

CANADA

R. T. NAYLOR. *Canada in the European Age, 1453-1919*. Vancouver: New Star. 1987. Pp. xv, 617. Cloth \$39.95, paper \$19.95.

This book has had a rough past. It will probably have a rocky future. R. T. Naylor refers in his acknowledgments to five years of wrestling with the Social Science and Humanities Research Council (SSHRC) of Canada to secure a grant-in-aid-of-publication, without which university presses in the country will not forward the work to the printer. Apparently, he never got them to the mat, and New Star, a small west coast publisher, took the text on spec. It is to be commended. But New Star and Naylor will no doubt soon face an avalanche of negative reviews.

Naylor is an economic historian who does not spend his time in the cliometrics club or the consulting circle. He writes, among other things, sweeping accounts of Canadian business that, in their Gustavus Myers-like raking through the muck, conform to no contemporary intellectual trend. (Naylor does report that Immanuel Wallerstein's "international systems" approach influenced the writing of this text, but it is surely the author's own rather loose and idiosyncratic reading of this overblown "theory" that prevails here.) Pillage, plunder, and pork barrels race and roll through Naylor's reading of the past; graft, corruption, and shameless self-interest line up on every page of his historical ledger. Refreshingly sardonic and elegantly sarcastic, he writes with wit and unusual antagonism to the powerful, and in this book the metafigures of international and

Canadian capital accumulation—Christopher Columbus, Comte de Frontenac, John A. Macdonald, J. P. Morgan—come in for Naylor's characteristic scorn for the personnel of acquisitive individualism's centuries-old army of marauders.

The book is a fast-paced tour through five centuries of Canadian economic history. The organizational premise is that the peculiarities of the country's development can be grasped only through an appreciation of Canada's status as one component in the pervasive struggle of the world powers to consolidate empire. It is an old story—given that the longstanding staples interpretation of Canadian history has always stressed international links and determinations—with a slightly new twist in the resolute stress placed on the global economy.

Naylor draws useful distinctions between early bullionism and mercantilism, explores how Great Britain's Industrial Revolution restructured possibilities in the colony, details the wars, conquests, and contests associated with French-English rivalry in the New World, examines emigration, colonization, and the connectedness of finance and state formation in the pre-Confederation years, looks at the influence of railroads and state consolidation in the industrial-capitalist ascendancy and orchestrated national unity of the mid-nineteenth century, and closes with the international rivalries, cyclical booms and busts, trade paths, and ultimate war of the epoch of high imperialism. The coming of what Naylor dubs an American age in the aftermath of World War I provides a convenient, if somewhat forced, termination of the study.

In a discussion of this sort, analysis of strengths and weaknesses, enthusiasms and antagonisms could go on for pages. Strong on his treatment of native peoples, Naylor is much weaker on labor and has little to say about women. Attentive to Canadian regions, he is at times a bit guilty of rehashing the obvious and cultivating trees in a way that makes it difficult to see the forest, an interesting problem in a book that strives so obviously to keep the big picture in sight. Based on a wide reading of secondary materials (and this is a study with no footnotes or primary research), the text is at times curiously selective in its use of sources. Naylor's problems with the SSHRC aside—which meant that a study completed in 1980 did not find its way into print until late 1987—it is rather difficult to explain why some titles from 1984, 1985, and even 1986 make it into the forty-page bibliography and others do not. These gaps matter because interpretations change and new evidence appears: in the case of labor, native peoples, and the making of industrial capitalism, Naylor's views are rooted at times in a

dated and debatable literature. He misreads the nature of nineteenth-century class struggle, is overly quick to imply the ecological morality of the Indian, and seems unaware of the extensive literature on proto-industrialization and household economy. Quite glaring, if easily understood, is the omission of any reference to Michael Bliss, Canada's best-known mainstream historian of business. But then Bliss's recently published *Northern Enterprise* (1987) declared early in its bibliographical note that Naylor's previous work was "factually unreliable, and was of no use in the preparation of this history." Tit for tat.

Naylor's text invites comparison with Bliss's book. Both are written for an audience that is not immersed in the archives—Naylor directing his study to students, Bliss aiming his at the business community. Neither makes much of a stab at being theoretical. Both focus, in part, on the relations of business and the state, although they of course read those connections differently. Competition turns the pages, in one case white-knuckled with rage, in the other benignly. They are comparable books; they are as different as night and day. Scholarship will not be pushed forward by either one, but teaching may be made a bit easier. I happen to prefer Naylor's work because it integrates individuals and historic process within a framework that makes some sense, whatever its interpretive lapses and minor errors. But the Canadian historical profession and the economics departments where the odd course that might use Naylor's text is taught are not likely to see things in this way. Nor are many other reviewers.

BRYAN D. PALMER
Queen's University

EVA-MARIE KRÖLLER. *Canadian Travellers in Europe, 1851–1900*. Vancouver: University of British Columbia Press. 1987. Pp. xv, 197. \$25.95.

This book describes the experiences and attitudes of Canadian travelers to Europe during the second half of the nineteenth century, a time when technological changes in travel, particularly the steamboat and railway, were making international travel more common. The book begins with a brief introduction to the literature. The journals of Fred C. Martin, a Woodstock lawyer, and Néré Gingras, a Quebec priest, are examined and compared in order to introduce the reader to the themes of the book. The nature of transatlantic travel and the characteristics of travelers are also examined, and photographs are included of those whose writings receive special attention. Comparisons are made between English and French-

speaking travelers, and special attention is given to the perspectives of women. Varied perceptions of European cities are presented, particularly London, Paris, and Rome, and the reactions of Canadians at world expositions are discussed to illustrate themes, such as English-French differences and notions of Canadian identity, which run throughout the book. In comparing Canadian experiences with those abroad, travelers also reflected on the characteristics of their origins, as well as their destinations, and, in doing so, revealed insights into Canadian attitudes both before and after Confederation.

As a professor of English, the author's main sources of information are the journals and publications of the travelers, and she has been diligent in searching out both published and unpublished documents, and articles in contemporary newspapers and magazines. The book is replete with long quotations, in both English and French, which constitute the evidence for the author's assertions and also make the writing turgid in places. There is little quantitative information of any kind and only limited reference to the academic literature on tourism. The author is aware of both the strengths and limitations of her materials. On the positive side, there is an abundance of sources that are rich with contemporary insights. On the negative side, it is easy to agree with the author that writers of books and journals are atypical, that many of the travelers were on business as well as on vacation and, in the absence of other information, one can only guess at the representativeness of their views.

The book is successful in elucidating and illustrating important topics, such as the perception of places, national identity, English-French similarities and differences, and gender contrasts in roles and perspectives. Some insight is also provided on temporal changes in such themes. However, the potential impact of the research is not realized fully because there is no clear statement of objectives, hypotheses, or conceptual framework. Similarly, the book lacks a conclusion: it ends following a discussion of world exhibitions that, admittedly, is designed to integrate some earlier ideas, but there is no explicit synthesis or summary. In the absence of a conclusion, readers must form their own judgments and are deprived of the insights of the author at a moment when they are likely to be most useful. Nonetheless, the number of books on the history of tourism is small, and this work is a solid contribution to the literature.

GEOFFREY WALL
University of Waterloo

LATIN AMERICA

CORNELIS CH. GOSLINGA. *The Dutch in the Caribbean and in the Guianas, 1680–1791*. (Anjerpublikaties, number 19.) Wolfeboro, N.H.: Longwood or Van Gorcum, Assen/Maastricht, The Netherlands, 1985. Pp. xii, 712. \$50.00.

In his study *The Dutch in the Caribbean and on the Wild Coast (1580–1680)* (1971), Cornelis Ch. Goslinga focused on a period that saw the Dutch in revolt against Spain and elbowing their way first into the Caribbean and later Brazil. Thus, his story was one of battles, piracy (depending on the point of view), and the dedication of the old Dutch West India Company to wreaking commercial havoc on the Iberians by, among other things, grabbing as much control of the slave trade as possible.

This sequel considers another century-long period of the Dutch presence in the Caribbean region. It deals with somewhat quieter (although by no means dull) times presided over by a new Dutch West India Company, and Goslinga begins his study with a brief survey of the organization and history of this company, whose life span encompassed the years 1675–1791.

He next examines the West India Company on the African coast, where, caught up in the usual international squabbling over trading turf, the company seems momentarily, at least, to have been just as aggressive as its predecessor. Much of the belligerence faded, however, with the passing of the *asiento* to the British in the wake of the War of the Spanish Succession, and following this the role of the company in the slave trade became, for the most part, one of collecting fees on slaves transported by others.

Goslinga's focus shifts at this point to the Americas and to a discussion of the normally strained relationship between company and colonists first in Curaçao and then in the Dutch Leewards. He next devotes two chapters to the Dutch slave trade within the Caribbean as well as trade generally with other islands and the North American mainland. A chapter on colonial society in the Dutch islands follows before Goslinga turns to the South American north coast to take up Surinam, the Surinam slave trade, and the Surinam maroons. Similar, although necessarily abbreviated, treatments follow for Essequibo, Demerara, and Berbice, the latter site of the great, albeit ultimately unsuccessful, slave rebellion led by Coffy.

The study closes with a comparative examination of colonial Willemstad and Paramaribo (the capitals of Curaçao and Surinam), an examination of the evolution of laws regulating Dutch slavery, and a lingering look at the last decades of the West India Company. Many of the themes dealt with

are common to the history of all colonies and colonial powers in the Caribbean—slavery, the slave trade, slave rebellions, monopolistic companies versus a planter and merchant elite, interisland trade and interloping, and defense, to name but a few. Goslinga's contribution is to treat these themes from the little-known Dutch viewpoint yet always to do so within the context of the larger international rivalries being played out on both sides of the Atlantic. In addition he is most sympathetic to the plight of the slaves, especially in Surinam and Guiana, where a small white minority abetted the conversion of slaves to maroons by its oppressive and extraordinarily cruel treatment of black people.

Another most useful feature of this study is the truly awesome amount of detail it deploys, much of it unearthed by the author in the General State Archives in The Hague and the record offices of other Dutch towns. Those with an appetite for numbers will find it satisfied by demographic statistics, slave-trade data (Goslinga raises estimates of slaves exported from Africa by the West India Company and provides the number imported by Surinam), production figures on sugar, coffee, cacao, and cotton, and balance sheets of the company worthy of any accounting firm. And this brief menu of hard data provided does not even begin to scratch the surface. The appendixes consist of another thirty-nine pages listing, among other things, the African ports of departure and the Caribbean ports of arrival of slaving vessels, the average prices of slaves sold at public auction, and slave deliveries to Surinam.

Anyone doing work on the slave trade, comparative slave societies, slave rebellions, or colonial governments will be grateful for this most welcome addition to the literature.

KENNETH F. KIPLE
Bowling Green State University

La esclavitud en Cuba. Havana: Editorial Academia. 1986. Pp. 279.

This collection does not provide a thorough review of the institution of slavery in Cuba. It does, however, offer a useful summary of the present state of historical research as well as a tantalizing glance at some types of documentation available to the serious researcher on slavery in Cuba. Based overwhelmingly on locally available (and mostly archival) sources, the essays range widely in themes, and basically they fall into two categories: essays about slavery and essays about sources. Some authors such as José Luciano Franco, Rafael López Valdés, Fe Iglesias García, and Gloria García are either well-known or established histo-

rians. The contents include an overview of slavery and the slave trade; ideas about Cuban slavery held by Cubans; the process of abolition; Russian writers on Cuba; archaeological sources for studying Cuban slavery; an examination of the sources used by José Antonio Saco in his unpublished study, "*La Historia de la esclavitud en las Antillas Francesas*" (A history of slavery in the French Antilles). Although occasional references are made to the earlier period of Cuban history, the focus lies on the later eighteenth and early nineteenth centuries.

A few of the essays are impressive and interesting. Fe Iglesias García offers an excellent synthesis of the process of disintegration in her brilliant essay, modestly called "*Algunas consideraciones en torno a la abolición de la esclavitud*" (Some considerations concerning the abolition of slavery). Offering a much simpler periodization than her colleagues Rafael López Valdés and Mildred de la Torre, she places the history of Cuban slavery in three phases—an initial phase up to around 1790, a mature phase between 1790 and the middle of the nineteenth century, and the decline thereafter—and makes the provocative observation that Cuban slavery was closely tied to the evolving stages of international capitalism and imperialism with profound consequences for the entire society. In a skillful blend of primary and secondary sources, she provides valuable new information on a variety of slave occupations, as well as their distribution geographically and by agricultural activity throughout the island. Her conclusions both complement and qualify those of Rebecca Scott's earlier study. Using documents from the Real Consulado and the Junta de Fomento, Gabino la Rosa Corzo has detailed the construction, duration, and destruction of more than sixty-two *palenques* or maroon refuges between 1800 and 1850, some with populations of several hundred runaways—although the typical *palenque* had a population of about fifty individuals.

Three other essays merit some attention. Mirtha González Moreno indicates that a valuable, much overlooked source for studying Cuban slavery lies in *Los Libros de registros de entrada y salida del Depósito de Cimarrones de la Habana* (Registers of entry and departure of runaways for the City of Havana) covering the period between the 1830s and the 1850s. Apart from detailed physical descriptions of the individuals detained, these registers contain information on owners (or, more accurately, presumed owners), costs of stay, diet, and tasks assigned. Unfortunately, the brevity of the presentation does not allow for many answers to the numerous questions raised about the source or the detainees. Orestes Gárciga offers a textual, meth-

odological, and historiographical appraisal of an unpublished manuscript of the celebrated Cuban writer José Antonio Saco; and Ángel García and Piotr Mironchuk review an extensive Russian literature on Cuba with special reference to observations about slavery. It is somewhat surprising that the Russian writers frequently make comparisons with slavery in the United States but remain strangely silent on serfdom in their homeland—at least in this essay.

Some of the other essays are quite disappointing. They are simple, narrow, and reflect little awareness of the extensive non-Cuban sources and stimulating discourses of the broader historiography. The annotated bibliography of Ernesto Ruiz has many deficiencies, including the neglect of outstanding books and articles by Arthur Corwin, Levi Marrero, and Rebecca Scott. The publication coincides with the centenary of the abolition of slavery in Cuba, although no direct mention is made of this. The volume is so poorly bound that it fell apart before I could complete it.

FRANKLIN W. KNIGHT
Johns Hopkins University

ALFRED N. HUNT. *Haiti's Influence on Antebellum America: Slumbering Volcano in the Caribbean*. Baton Rouge: Louisiana State University Press. 1988. Pp. xiv, 196. \$25.00.

Only in recent years have appreciable numbers of American scholars begun to take Haiti seriously. Anthropologists long recognized the vitality of Haitian history and culture and its significance for the study of society in the Americas, but the academic community has often marginalized these anthropologists' contributions by focusing on the exotic elements in their discourse and ignoring the provocative questions that they were raising. Now historians have joined anthropologists and other social scientists in reassessing the black republic.

Alfred N. Hunt's contribution to this literature affords a fresh look at the impact of Haiti and the Haitian revolution on the antebellum United States. Hunt offers the attractive thesis that the American South represented the northern extremity of Caribbean culture, wedded as it was to slavery, plantation agriculture, and a dependent economy. Viewed in hemispheric rather than national perspective, the South's singular character and its response to events in Haiti are more readily understood.

The French and Haitian revolutions and the United States' ambivalence toward both form the backdrop for an examination of the key events of the epoch. Hunt describes the refugees who, in the wake of these conflicts, settled in Louisiana and

other seaboard states and made vital contributions to North American life. He also details the image of Haiti in the mind of the antebellum South, and his rendering of southern perceptions of Toussaint Louverture is particularly acute. The black emigration and colonization schemes of the late antebellum period are not neglected; the activities of those who saw Haiti as a possible homeland for expatriated freedmen are explored, as is the nineteenth-century black American viewpoint.

Hunt covers ground only barely sketched out by such historians as David Brion Davis, whose study of slavery in the context of revolution was based on the tacit assumption that only European and white American historical actors are important. Hunt nevertheless does not completely account for the sheer amplitude of the Haitian/Saint Domingan influence on the American South and therefore does not fully sustain his thesis that the South can be understood as an area with a Caribbean heritage. Such an assertion cannot be maintained without a more detailed examination of black immigrants than has been made here. The vast majority of the refugees from the Haitian revolution were blacks or of mixed race. These get rather short shrift as the author concentrates on the white émigré planters and merchants whose culture was arguably more French than Caribbean and who assimilated into American life with relative ease. Such assimilation was not so readily accomplished by Creole nonwhites, who profoundly influenced the South's—and the nation's—literature, music, and popular culture. Their very existence haunted society in Louisiana and stirred the troubled waters of U.S. race relations.

Hunt also seems at times to accept too easily the common notion that, despite the impressive achievements of Haiti's revolution, its subsequent history has been so much a failure that study of these later years can be greatly compressed. We learn little about Haiti's impact during the period between the revolution in 1804 and the antislavery and sectional agitation in the United States in the 1850s. While these shortcomings are significant, they do not detract from the importance of this study as a sound introduction to a rich, fascinating, and hitherto neglected subject.

BRENDA GAYLE PLUMMER
University of Minnesota

FERNANDO PICO. 1898: *La guerra después de la guerra*. Río Piedras, P.R.: Huracán. 1987. Pp. 215.

This is Fernando Pico's fifth book on Puerto Rico since 1979. His impressive scholarly contributions include a superb synthesis of island history, two

monographs on nineteenth-century workers and farmers in the coffee uplands, and a study of social marginality among the highland poor after the U.S. invasion.

Picó now turns to the analysis of banditry during and after the brief Puerto Rican campaign of the Spanish-American War. It is essentially the story of the *partidas sediciosas*, armed groups that terrorized the Puerto Rican countryside, particularly the coffee areas, in 1898 and 1899. Chiefly active during the aftermath of the invasion (July to October 1898), the *partidas* focused their anger on peninsular Spanish merchants and landowners. The bandits battered or killed their victims, and they almost invariably destroyed property and business records.

The looting of financial ledgers was a uniform component of the violence. Believing that this action reveals the social roots of banditry and underscores its retaliatory character, Picó pays considerable attention to its symbolic and concrete meanings. For want of banks and other financial institutions, workers and smaller farmers had tended to accumulate large debts with those who controlled the flow of cash and goods, that is, the merchants and large planters. The coffee boom of the late nineteenth century exacerbated such widespread indebtedness and impoverished a growing proportion of the highlanders. Many Creoles increasingly saw a connection between their own destitution and the monopoly of wealth and power exercised by immigrant merchants, planters, and appointed local officials. As the usual mechanisms of social control broke down during the military campaign, there occurred spontaneous outbursts of resentment against the perceived villains. Thus, in Picó's view, the *partidas* were the most violent expression of a widely felt anti-Spanish sentiment, which had simmered for decades underneath a façade of colonial contentment and civility.

Although few of these arguments are entirely new, the book makes several valuable contributions. First, it places resistance alongside accommodation at the core of Puerto Rico's turn-of-the-century history. Second, it documents the scope, chronology, and social roots of the violence of 1898. Although Picó does not approach banditry in theoretical or comparative terms, his thoughtful typology of the *partidas* sheds light on the tensions that undercut Puerto Rican society at the end of the nineteenth century. Finally, the author reconstructs, with the help of U.S. military records, previously unexplored aspects of the invasion. He shows, for instance, how General Nelson Miles, lacking confidence in his predominantly volunteer troops, sought to avoid military clashes with the Spaniards. Relating the avoidance of

military action to the ease with which the invaders reestablished authority at the local level, Picó suggests that military concerns may have prevented the spread of social violence. Although this does not fully explain the cordial reception accorded the U.S. troops, it advances our understanding of the ways in which military conduct may have averted a greater rampage, perhaps directed at the invading troops as well as at the defeated Spaniards.

FRANCISCO A. SCARANO
University of Connecticut

NIGEL DAVIES. *The Aztec Empire: The Toltec Resurgence*. (The Civilization of the American Indian Series, number 187.) Norman: University of Oklahoma Press. 1987. Pp. xiv, 341. \$39.50.

In the third volume of his trilogy on the Toltecs, Nigel Davies deals with the achievements of the Aztecs, successors to the Toltec power, in the relatively brief period 1428–1521. A large part of the text is devoted to efforts to unravel the many problems, large and small, that complicate the study of Aztec history, thanks to the atmosphere of myth that surrounds its earlier stages and the frequently contradictory versions of the Indian and Spanish sources. One can only commend Davies for the industry with which he has worked through these sources and the skill with which he has examined conflicting evidence to reach conclusions that often appear reasonable and convincing. I find especially satisfactory his handling of one of the major problems of Aztec history, the question of the identity, role, or even the very existence of the famous Tlacaélel, Aztec king-maker and *éminence grise* over several reigns. Davies has managed to strip away the legendary elements from his life while confirming the reality and importance of Tlacaélel as a policy-maker and architect of Aztec greatness.

I find Davies less satisfactory when he attempts to explain Aztec social organization or when he deals with such large theoretical problems as the motivations of the Aztecs' rise to power. An implausible foe of historical materialism and "nomothetic explanations" in general, Davies himself is free with large, sweeping generalizations that many will find dubious. Thus Davies rejects as meaningless "any attempt to divide [Aztec] society into the privileged and unprivileged" and the application of such terms as "class warfare" and exploitation to Aztec society (pp. 125–27). But it is difficult not to find more than a whiff of "class hatred" in the following passage by one of Spanish missionary Bernardino de Sahagún's Aztec informants: "the merchants were those who had plenty,

who prospered; the greedy, the well-fed man, the covetous, the niggardly, the man who controlled wealth and family" (*General History of the Things of New Spain*, Book 7 [1853], 23). In his assessment of the motivations and causes of the Aztec rise to power, Davies also rejects the importance of ideology as a factor and offers, as interesting asides, the claim that "in no true sense can modern American democracy" be regarded as an ideology and that "whatever the motives that moved England, they cannot remotely be portrayed as ideological" (pp. 275–76). Among the explanations of the Aztec success singled out by Davies as especially relevant are "Nietzsche's will to power" and the Aztec Spartan virtues, "in brief, sheer guts" (p. 288). Presumably, the Chalcans, Tolucans, and the other peoples conquered by the Aztec juggernaut were lamentably deficient in this vital element of "sheer guts."

BENJAMIN KEEN
EMERITUS
Northern Illinois University

LOUIS NICOLAU D'OLWER. *Fray Bernardino de Sahagún (1499–1590)*. Translated by MAURICIO J. MIXCO. Foreword by MIGUEL LEON-PORTILLA. Salt Lake City: University of Utah Press. 1987. Pp. xviii, 201. \$25.00.

In the first decades after the Spanish conquest of Mexico, a group of friar-humanists, chiefly Franciscans and Dominicans, created a genuine school of ethnography devoted to making an inventory of the rich content of Aztec culture. Although the primary and avowed aim of these missionaries was to arm workers in the Indian vineyard with the knowledge they needed to discover the concealed presence of pagan rites and practices, intellectual curiosity and appreciation of the material, social, and artistic achievements of the vanished Aztec empire also played a part. By general consent, the towering figure in that group of scholars was the Franciscan Bernardino de Sahagún. Amid the vast corpus of Sahagún's writings, some of which still remain unpublished, the most important is his *General History of the Things of New Spain* (1550–82); students have only begun to mine the extraordinary wealth of ethnographic materials in Sahagún's work. In recent years the research of many scholars has shed new light on various aspects of Sahagún's achievement; an example is the collaborative volume, *Sixteenth-Century Mexico, The Work of Sahagún*, edited by Munro S. Admondson (1974). The most comprehensive, detailed study of Sahagún's life and works, however, remains the book under review. Despite the somewhat wooden

quality of Mauricio J. Mixco's translation, it serves the purpose of making Luis Nicolau D'Olwer's excellent work known to a wider reading public in the English-speaking world.

Two opening chapters deal with Sahagún's departure from Spain in 1529, his early years in Mexico, and his role in the founding of the famous Colegio de Santa Cruz at Tlaltelolco, designed to provide a Christian and humanist education to the sons of the Indian nobility. Succeeding chapters trace the stages in the evolution of Sahagún's vast scholarly enterprise, which employed a scientific methodology that differed little from that of modern anthropological field work. As part of the preparation for writing the Nahuatl text of his *History*, Sahagún drew up a *minuta*, or questionnaire, that he used to elicit information from a group of Indian elders at Tepepulco; the results of this inquiry were later revised, corrected, and expanded with the collaboration of another group of Indian elders at Tlaltelolco. Another chapter deals with Sahagún's Spanish translation, or rather paraphrase, of the Nahuatl text of the *History* and with the peremptory royal order for the seizure of Sahagún's manuscripts and their dispatch to Spain—a crushing blow to the great scholar. D'Olwer's discussion of this event suffers from his failure to link it to a larger political and social context: the triumph in the Spanish court of an anti-Indian climate of opinion and policy of which the confiscation of writings on Indian antiquities and the ban on future writings on that subject were only one aspect. The next chapter deals with a related subject that also involved Sahagún: the efforts of the Inquisition to forbid all translations of the Scriptures into Indian languages. There follows a chapter that describes Sahagún's efforts, using the fragmentary materials that remained in Mexico, to revise or refine parts of his great work.

Chapter 9, the longest and perhaps most interesting of all, carefully analyzes Sahagún's methodology and his ideology in its relation to the Franciscan dream of creating a New Jerusalem in Mexico. A final chapter, "Exhuming the *Historia*," traces the vicissitudes of the long and still unfinished effort to track down and bring to light the various parts of the vast Sahagún corpus. The translation is preceded by an informative foreword by Miguel León-Portilla dealing with D'Olwer's life and scholarly achievements; the translator has provided a useful glossary of Spanish and Indian terms for the English edition.

BENJAMIN KEEN
EMERITUS
Northern Illinois University

DORIS M. LADD. *The Making of a Strike: Mexican Silver Workers' Struggles in Real del Monte, 1766–1775*. Lincoln: University of Nebraska Press. 1988. Pp. x, 205. \$21.95.

This book is a major, innovative achievement in Mexican history. Through a close examination of the intense labor conflicts at the mines of Real del Monte in the 1760s, Doris Ladd has taken Mexican social history to new and exceptionally revealing depths. Recently, we have learned much about the conditions of labor; now, we discover the debates, proclamations, and the often combative actions of a community of workers who understood their power as producers of the silver that made Mexico the most valuable European colony of the late eighteenth century. Ladd's work also demonstrates that social history cannot exclude the state. It was, after all, interventionist colonial bureaucrats who compiled the testimony that allows the historian to reconstruct the workers' world. And the analysis reveals that the relationship between mining entrepreneur Pedro Romero de Terreros and the mine workers of Real del Monte was mediated by a state that had a primary interest in the maintenance of labor peace and silver production—to keep bullion flowing into Crown coffers.

Using a forceful, accessible narrative, Ladd details the conditions of production and the complexities and dangers of labor at Real del Monte. She explains how Romero de Terreros's attempts to eliminate *partidos* (skilled workers' shares of silver ore) and to lower wage rates led to a series of confrontations and work stoppages. The parish priest stepped in and mediated the most explosive crises—and then the colonial authorities entered the fray to mediate an eventual resolution. Recognizing the importance of silver production and aware of the structural shortage of labor that sustained the workers' bargaining power, the authorities gave in to the workers on most issues in order to bring the mines back into production.

In exploring the aftermath of the strike, Ladd portrays Romero de Terreros as driven by a combination of arrogance and paranoia to resume production and punish all who opposed him. She follows with compassion the heroic yet tragic tale of the parish priest who mediated conflicts at their most heated moments—saving Romero de Terreros's life—only to be broken later by the entrepreneur who saw the priest's mediation as capitulation to unreasoning and unreasonable workers.

Ladd sees the workers and the priest in ways that Romero de Terreros would not. She portrays a grasping entrepreneur trying to profit by slashing the customary earnings of the workers. She sees a community of workers clearly aware of their

importance to the colony's pivotal industry and ready to organize, protest, and strike to maintain their traditional remuneration. She finds a learned priest quick to risk harm in order to mediate the first and most explosive confrontations. And she emphasizes the critical role of the state in mediating a final solution that left Romero de Terreros inordinately rich, the workers confirmed in their traditional *partidos* and pay scales—and the priest expelled from his post, sacrificed to the entrepreneur's arrogance.

This is a history in which structure and humanity are brilliantly blended. There is no more revealing analysis of state and society in Mexico as the colony moved toward the conflicts of the independence era.

JOHN TUTINO
Boston College

THOMAS D. SCHOONOVER, editor. *Mexican Lobby: Matías Romero in Washington, 1861–1867*. Lexington: University Press of Kentucky. 1986. Pp. xviii, 184. \$21.00.

Matías Romero, liberal Mexican statesman, served the Benito Juárez administration in Washington during most of the crucial period, 1861–1867, moving through the ranks from legation secretary to minister. He deserved the promotions as well as this intriguing new study. A cheeky, young outsider, he attached himself to the Radical Republican cause, hoping to precipitate a forceful implementation of the Monroe Doctrine in Mexico.

From thousands of Romero's official letters, Thomas D. Schoonover has selected, translated (smoothly), and edited about 125 dispatches from Romero to his government. Most of these reports concern his schemes to obtain financial help for Mexico and remove the French troops supporting Prince Maximilian. Seeking U.S. intervention for the removal of the Austrian, Romero spent vast sums contacting and entertaining congressmen and generals—sometimes dining two hundred wheeler-dealers at once. He collared Washington's powerful, told them what he needed, then wrote the boss about how well he had done the job. Puzzles abound: what was the source of his money; how did he become General Ulysses S. Grant's best friend; why did Postmaster General Montgomery Blair leak secret cabinet discussions to him daily; how could Romero literally move to the head of the line of hundreds of suppliants waiting to see presidents Abraham Lincoln and Andrew Johnson, neither of whom liked him?

Unless he was the grandest of liars (and I agree with Schoonover that the assumption is unlikely), this consummate lobbyist could “drop in anytime”

to badger Thad Stevens, Ben Wade, Zach Chandler, Elihu Washburne, Schuyler Colfax, Henry Winter Davis, and many others. Romero wrote legislation for them; he campaigned for John C. Frémont in 1864 for president; but he failed utterly to impress one man—Secretary of State William H. Seward.

If I have a problem with this book, it is that the editorial work seems to elevate Romero and downgrade Seward. Too little is said about Europe's delight at taking advantage of Uncle Sam's worst trial, and a casual reader could lose perspective. Not just Mexico was at risk; the Dominican Republic, Chile, possibly Peru faced armed intervention precisely *because* of our Civil War. Seward saw the limits of power and counted on diplomacy, Europe's usual strife, and a Union victory. His policy was a gain for both Mexico and the United States.

Meanwhile, Romero proposed many cockamamie schemes for the use of thousands of veterans—blue and gray—in a great private army. Grant, Robert E. Lee, Philip H. Sheridan, Jefferson Davis—any big name could lead these men, drive out the French and save Juárez (although Sheridan worried that Americans could not survive on tortillas). For pay, the men would get the historic inducement, land grants. Romero casually conceded that Mexico might lose a few states in the north! One hesitates to contemplate our history, had regiments of land and woman-hungry Rebs and Yankees reached Chihuahua. Instead of attacking Seward's obduracy, Romero should have thanked him for saving Mexico from its friends.

Mexican Lobby provides provocative and speculative reading, not an easy achievement for an edited book.

THOMAS L. KARNES
Arizona State University

MARIE-DANIELLE DEMELAS and YVES SAINT-GEOURS.
En Amérique du Sud au temps de Bolívar 1809–1830.
(*La vie quotidienne.*) Paris: Hachette. 1987. Pp.
251. 100 fr.

Written primarily for the "grande publique," this volume attempts to present a general survey of independence movements in Spanish South America. It does not claim to introduce new primary source material on the subject but rather has as its goal a readable synthesis of secondary work on the period. Although it appears in "La vie quotidienne" series, it does not treat daily life except as a brief background to political events. It concentrates neither on Bolívar nor on any of the other independence leaders of the period. Rather, this volume presents a survey of the complex political and military events that constituted the

early nineteenth-century South American revolutions.

Not surprisingly, the book has a decidedly French point of view. Indeed, at times, the French seem to be the only non-Spaniards worthy of mention. This makes the scant reference to the Bourbon-inspired reforms of the late eighteenth century all the more noteworthy. The same vision informs the brief bibliography and notes, which are notable for the scarcity of citations to works in English. Most astonishing is the lack of reference to the masterly study by John Lynch, *The Spanish American Revolutions, 1808–1826* (1973), which remains the best one-volume synthesis on Latin American independence.

Nonetheless, the authors have succeeded in creating a lively picture of the independence struggles in all their complexity, ranging from urban insurrection to rural campaigns, from civilian strategies for survival to the eventual victors of these devastating wars.

SUSAN MIGDEN SOCOLOW
Emory University

ROBERT EDGAR CONRAD. *Children of God's Fire: A Documentary History of Black Slavery in Brazil.* Princeton: Princeton University Press. 1983. Pp. xxviii, 515. Cloth \$55.50, paper \$18.50.

My experience with published collections of documents has been that they are often useful as research tools but seldom make for compelling reading. This volume is a welcome exception to that rule. Its various texts offer a comprehensive and multidimensional portrayal of the history of Brazilian slavery, written in vivid, direct language that makes the book a pleasure to read.

Robert Edgar Conrad's goal in this collection is to put the final nails in the coffin of the Tannenbaum thesis by demonstrating once and for all that Brazilian slavery was every bit as cruel and degrading an institution as its North American counterpart and quite probably even more so. This is not a new argument; if anything, it forms the current scholarly consensus among students of comparative slavery. Conrad's contribution is to allow the participants in the three-hundred-year drama of Brazilian slavery to make the case themselves, sometimes intentionally and sometimes unintentionally. Some of the most damning material comes from defenders of slavery, who, in making the strongest case they can for the institution, unwittingly betray its barbarism. For example, one senator from Minas Gerais, noting the improvements in the treatment of slaves after the abolition of the slave trade in 1850, observes that before 1850 only 5 percent of slave children survived to

adulthood; after 1850, he says, the figure rose to 25–30 percent.

The documents cover various aspects of slavery: physical conditions of servitude on the plantations and in the city, the legal status of slaves and free blacks, the relationship of the Catholic church to slavery, slave resistance and rebellion, and the struggle for abolition. Considerable complexity and variation emerge in each of these areas. Within a single urban neighborhood or on a single plantation, slaves worked at different kinds of jobs, had different kinds of relationships with their masters, and lived at different levels of material well-being. While some documents report on the almost casual violence and brutality of the slave trade and the slave regime, others look at efforts by enlightened masters to treat their workers with humanity and consideration. And, while the church accepted the legitimacy of slavery and in fact bought, sold, and held slaves in large numbers, its priests discussed with considerable sensitivity and empathy the special spiritual and physical needs of their slave parishioners and how best to minister to those needs.

But such expressions of kindness and humanity were the exception rather than the rule and tended to be overpowered and swept aside by the inherent brutality of slavery as an institution. Indeed, one of the most poignant and compelling of the book's themes is the centuries-long effort by conscience-stricken Brazilians to find some way to reconcile the cruelty and injustice of slavery with the Christian doctrine on which their civilization supposedly was based. The impossibility of resolving this conflict is suggested by an 1883 court case in which the judges reluctantly concluded that, while the rape of a twelve-year-old slave girl by her master was morally deplorable, under the laws governing slavery it was not an actionable offense.

Some readers may criticize the book for its relative lack of testimony by slaves themselves (only seven documents in a collection of more than one hundred). As Conrad makes clear, this absence is the result of the extreme paucity of such material in Brazilian archives and libraries, which is in turn a result of much lower levels of literacy among both slaves and free people in Brazil as compared to the United States. If anything, Conrad has done an extraordinary job of searching out, selecting, and translating a rich body of contemporary testimony that will give readers an immediate and powerful sense of what it meant to live in a society based on slavery and plantation agriculture. Even historians well read in these issues can read this book with profit; for under-

graduates and the general public, it will be invaluable.

GEORGE REID ANDREWS
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MARY C. KARASCH. *Slave Life in Rio de Janeiro, 1808–1850*. Princeton: Princeton University Press. 1987. Pp. xxv, 422. \$85.00.

This handsomely printed and richly illustrated book details the life of slaves in Rio de Janeiro during the first half of the nineteenth century. Mary C. Karasch begins by presenting the most precise study of their African origins heretofore available. To do so, she draws on a number of sources that include ethnic or geographical data for more than four thousand slaves and then identifies the locations in Africa to which the documents refer, adding learned commentary on the slave trade. She has combed more than two hundred travelers' accounts for descriptive references to slaves and searched the Brazilian National Archives and the Archive of the City of Rio de Janeiro for information, both literary and numerical, on slave life. This research enables Karasch to present full chapters on slaves' labor (whether as porters, stevedores, or street cleaners, whether artisans or servants, whether unskilled, skilled, or supervisory), on their punishments, on their clothing and diet, on their art, music, songs, dances, festivals, and religions, as well as on their resistance to their enslaved condition. She shows that some labored as industrial workers at the Ipanema ironworks—although the site was near Sorocaba in the state of São Paulo and not in the city of Rio de Janeiro as she indicates. Not the least achievement is the use of records from a charity hospital to examine the attributed cause of death for almost a thousand slaves, establishing that among the immediate causes the most frequent were tuberculosis, dysentery, and pneumonia. The result of her painstaking labor is a great wealth of information for the historian interested in almost any dimension of the urban slave experience.

In contrast to those scholars who are preoccupied with constructing broad theories or exploring the significance of slavery, Karasch concentrates on searching for facts. It is not her purpose, she says, to make "generalizations . . . but to . . . describe" (p. xxiii). One result of this approach, I suppose, is to allow readers the freedom to draw their own interpretations, ponder the possible meanings of these facts, and raise interpretive questions. What does the imprisonment of slaves by public authorities and at the request of masters suggest regarding masters' understandings of slaves' economic function, the nature of their

control, the psychology of the masters, or the use of cultural symbols? What does the practice of paying slaves "wages" tell us about the line between slavery and freedom and its significance for both slaves and freed? How are we to explain the improvement in slave diets during the course of the nineteenth century? Karasch's book provokes one to ask these and many other questions, but it attempts few answers.

Unfortunately, her suggestion in the introduction of some overall arguments hinders more than it helps. There Karasch tells us that one of her purposes is to counter "the Freyre thesis of benevolent masters whose gentle treatment made their slaves' lot in Brazil less rigorous than in North America" (p. xix). Insofar as Gilberto Freyre in the 1930s actually made a case for gentle treatment, his claim has been so thoroughly discredited by authors such as Stanley Stein (*Vassouras*, 1959), Marvin Harris (*Patterns of Race*, 1964), and a host of Brazilian writers that it no longer seems an issue for debate. The sufferings of slaves as she describes them are witness enough to brutality, without the need to address an outdated historiographic issue. On the other hand, Brazilian experience as described by Karasch may indeed surprise historians whose primary work has been on North American slavery; they may find it striking to learn of the frequency of manumissions, the number of those who practiced skilled occupations, or the presence of blacks within the military and the church hierarchy. Karasch draws particular attention throughout her book and with great effect to the predominance of Central Africans among the slaves imported to Rio de Janeiro; but, again, the introduction misleads in asserting that the common view is of a West African origin. Hers has been the general understanding at least since 1939 when Artur Ramos wrote *The Negro in Brazil*. Such claims detract from her achievement.

Two issues, in particular, are illuminated by Karasch's data. First, whereas many Brazilian specialists have asserted that the freeing of slaves has been exaggerated, benefiting mostly women and children, whose value was low, and that masters tended to favor with freedom mainly those born in Brazil to whom masters felt emotional ties, she shows that, by 1849, 13 percent of the African-born in Rio had been freed, that these Africans accounted for 71 percent of the freed, and that 44 percent of the freed were male. One is left to speculate on how such practices affected the slaves' understanding of their fate. Second, Karasch demonstrates that slaves imported from Africa were significantly younger than we had believed: 37 percent under age fifteen and 67 percent under twenty. As she emphasizes, this fact greatly enhanced the masters' ability to mold and hence

control them. An important contribution, this book will be regularly consulted for years to come as a reference for the study of slavery.

RICHARD GRAHAM
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WARREN DEAN. *Brazil and the Struggle for Rubber: A Study in Environmental History*. (Studies in Environment and History.) New York: Cambridge University Press. 1987. Pp. xv. 234. \$29.95.

Historians rarely attend to biological factors in explaining economic development. They focus on the interplay of values, technology, markets, interests, organizations, and quirks of leadership. The neglect of biology is not surprising because social explanations have generally been fruitful. Furthermore, advanced societies have been quite successful in overcoming the natural limitations to material welfare that haunted the Malthusians. Nevertheless, sociobiologists and ecologists have attempted to refocus the attention of social scientists on the limits biology places on human endeavor. Most attempts have been rightly refuted as simplistic.

Social forces, however, cannot explain everything. Such vagaries in natural systems as disease and weather have had a profound impact on human affairs. A series on environmental history is welcome, but using biological data and concepts is perilous. Since most social scientists are poorly trained in natural science, they are liable to apply its concepts naively.

Readers of this tour de force need not worry. In the introduction, Warren Dean lays out the basic facts. The world's highest yielding rubber plant, *Hevea brasiliensis*, is sparsely distributed in its indigenous Amazonian habitat. Because of expanding demand for automobile tires at the turn of the century, rubber prices soared, and Amazonia boomed. Soon plantations in British Malaya, the Dutch Indies, and French Indochina, rooted in smuggled *Hevea brasiliensis* stock, began yielding. While Brazilian production lagged, Southeast Asian plantations expanded rapidly. Rubber prices fell, with devastating effect on Amazonia. Successive plantation schemes in Brazil by the government and foreign entrepreneurs flourished. Brazilian rubber now contributes marginally to world supply.

Dean poses four questions: How was Brazil's monopoly lost? Why did Brazil not respond to the Asian challenge? Why could technology not overcome environmental limitations to Amazonian cultivation? Why have other new crops been successfully cultivated in Amazonia but not the native *Hevea brasiliensis*?

These questions are usually posed in social scientific terms by both scholars and Brazilians themselves. The loss of monopoly resulted from the perfidy of an English rogue, Henry Wickham, who smuggled rubber seeds to Kew Gardens. The Brazilian government failed to respond to the collapse of Amazonia because falling rubber prices benefited industrial buyers in southeastern Brazil. American and Brazilian cultivation schemes failed because of poor management, insufficient scale, and lack of long-term commitment. Unlike other new crops, advances in rubber cultivation threaten the marketing system that buttresses the archaic regional elite.

By offering ecological answers to these questions, Dean's work is innovative. Despite significant investments in rubber cultivation, an Amazonian leaf fungus, *Microcyclus ulei*, has proven an insuperable barrier to productivity. The blight is destructive to neither sparsely distributed trees in Amazonia nor closely spaced trees in Asian plantations. In lucid prose, Dean describes how the fungus proliferates and destroys. His discussion of the adaptation of pests to new conditions raises the possibility of Asians suffering a similar fate. In his sophisticated integration of ecological concepts into a socioeconomic history of rubber cultivation, Dean sets a high standard.

MARTIN T. KATZMAN
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IRENE SILVERBLATT. *Moon, Sun, and Witches: Gender, Ideologies, and Class in Inca and Colonial Peru*. Princeton: Princeton University Press. 1987. Pp. xxxiii, 266. Cloth \$39.50, paper \$14.50.

Rare is the book in Andean or Mesoamerican ethnohistory that gives equal attention to the pre-conquest and postconquest periods, attempting to treat the two as a continuum. The present publication does so and on that count alone deserves praise. Irene Silverblatt, a cultural anthropologist, devotes an equal number of pages to each period, following her themes through all three colonial centuries, and, even more important, she seems to have given the two epochs an equal amount of research and thought.

As Silverblatt declares (p. 213), "this book tells a partial tale." Narrowed to the well-defined theme announced in its subtitle, the study is free to range widely in other respects; it combines fresh primary research in an impressive variety of sources with synthesis of and comment on previous relevant scholarship. Extensive reliance on R. T. Zuidema, Karen Spalding, and others does not prevent the emergence of a strikingly new picture. We are

shown a thoroughgoing mental organization of the indigenous Andean world into parallel, complementary female and male halves, in every aspect from the gods and natural phenomena through kinship and work to the articulation of the larger society and polity.

Silverblatt then goes on to show how the symbolism of gender was used by the Incas in the creation and maintenance of their empire. In a particularly enlightening section, she demonstrates that the famous "virgins of the sun" were a logical extension into a larger arena of traditional Andean notions about conquest and gender. She leaves the reader fully convinced that gender considerations were a normal part of Inca politics, but the contrasts she makes between Inca practice and earlier or more local custom tend to be extremely speculative. She follows a venerable tradition of idealizing and simplifying pre-Inca organization; she underestimates the complexity of pre-Inca kingdoms and their degree of autonomy in Inca times, and she overestimates the newness of Inca departures, seeing them as policy manipulation invented for the occasion rather than as natural, spontaneous developments within the already existing Andean tradition. In any case, so little is known about the pre-Inca sociopolitical situation that the statements made about Inca innovations are tantamount to simple assertions.

As to the postconquest portion of the book, despite the author having done so much to unite the disciplines of anthropology and history, a discrepancy remains. Silverblatt is simply far less knowledgeable about the postconquest situation and especially about postconquest Spanish Peruvian society, the primary outside influence on indigenous Andeans. The partisan statements of the native chronicler Huaman Poma de Ayala are given excessive credence and prominence. In this context, or lack of one, it is hard to attach great value to the author's statements that women, or women of a certain type or sector, were particularly hard hit by certain postconquest developments. The highlight of this part of the book is a well-researched section on indigenous witchcraft of the middle colonial period, dominated apparently by women who were maintaining traditional community standards and values yet, on the other hand, were taking up a role that European expectations came close to creating. But again the necessary context is lacking. Indeed, most of the problems with this valuable, intelligent study go back to the difficulty of properly investigating a single specialized theme whose general context is not yet well understood.

JAMES LOCKHART
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Collected Essays

These volumes, recently received in the AHR office, do not lend themselves readily to unified reviews; the contents are therefore listed. Other similar volumes that are amenable to reviewing will be found in the review section.

JO BLATTI, editor. *Past Meets Present: Essays about Historic Interpretation and Public Audiences*. Washington, D.C.: Smithsonian Institution. 1987. Pp. x, 169. Cloth \$19.95, paper \$11.95.

JO BLATTI, *Past Meets Present: Field Notes on Historical Sites, Programs, Professionalism, and Visitors*. PEIRCE LEWIS, *Taking Down the Velvet Rope: Cultural Geography and the Human Landscape*. ELIZABETH COLLINS CROMLEY, *Public History and the Historic Preservation District*. MICHAEL WALLACE, *The Politics of Public History*. JANE GREENGOLD, *What Might Have Been and What Has Been—Fictional Public Art about the Real Past*. MICHAEL J. ETTEMA, *History Museums and the Culture of Materialism*. PATRICIA LAYMAN BAZELON, *Photographs*. BARBARA FAHS CHARLES, *Exhibition as (Art) Form*. IRENE U. BURNHAM, *So the Scams Don't Show*. MARY ELLEN MUNLEY, *Intentions and Accomplishments: Principles of Museum Evaluation Research*. WARREN LEON, *A Broader Vision: Exhibits That Change the Way Visitors Look at the Past*. MICHAEL H. FRISCH and DWIGHT PITCAITHLEY, *Audience Expectations as Resource and Challenge: Ellis Island as Case Study*.

DENIS COSGROVE and STEPHEN DANIELS, editors. *The Iconography of Landscape: Essays on the Symbolic Representation, Design, and Use of Past Environments*. (Cambridge Studies in Historical Geography, number 9.) New York: Cambridge University Press. 1988. Pp. ix, 318. \$49.50.

STEPHEN DANIELS and DENIS COSGROVE, Introduction: Iconography and Landscape. PETER FULLER, The Geography of Mother Nature. DOUGLAS DAVIES, The Evocative Symbolism of Trees. STEPHEN DANIELS, The Political Iconography of Woodland in Later Georgian England. JOHN LUCAS, Places and Dwellings: Wordsworth, Clare, and the Anti-Picturesque. HUGH PRINCE, Art and Agrarian Change, 1710–1815. DAVID FRASER, "Fields of Radiance": The Scientific and Industrial Scenes of Joseph

Wright. TREVOR R. PRINGLE, The Privation of History: Landseer, Victoria, and the Highland Myth. BRIAN S. OSBORNE, The Iconography of Nationhood in Canadian Art. G. MALCOLM LEWIS, *Rhetoric of the Western Interior: Modes of Environmental Description in American Promotional Literature of the Nineteenth Century*. MARK HARRISON, Symbolism, "Ritualism," and the Location of Crowds in Early Nineteenth-Century English Towns. PENELOPE WOOLF, *Symbol of the Second Empire: Cultural Politics and the Paris Opera House*. ERIC GRANT, *The Sphinx in the North: Egyptian Influences on Landscape, Architecture, and Interior Design in Eighteenth- and Nineteenth-Century Scotland*. DENIS COSGROVE, *The Geometry of Landscape: Practical and Speculative Arts in Sixteenth-Century Venetian Land Territories*. J. B. HARLEY, Maps, Knowledge, and Power.

LEON LIPSON and STANTON WHEELER, editors. *Law and the Social Sciences*. New York: Russell Sage Foundation. 1986. Pp. viii, 740. \$65.00.

LEON LIPSON and STANTON WHEELER, Introduction. SALLY FALK MOORE, Legal Systems of the World: An Introductory Guide to Classifications, Typological Interpretations, and Bibliographical Resources. RICHARD D. SCHWARTZ, *Law and Normative Order*. EDMUND W. KITCH, *Law and the Economic Order*. MARG GALANTER, *Adjudication, Litigation, and Related Phenomena*. DAVID R. MAYHEW, Legislation. JEFFREY L. JOWELL, Implementation and Enforcement of Law. JACK P. GIBBS, Punishment and Deterrence: Theory, Research, and Penal Policy. RICHARD L. ABEL, Lawyers. STEWART MACAULAY, Private Government. AUSTIN D. SARAT, Access to Justice: Citizen Participation and the American Legal Order. PHOEBE C. ELLSWORTH and JULIUS G. GETMAN, Social Science in Legal Decision-Making. SHARI SEIDMAN DIAMOND, Methods for the Empirical Study of Law.

SUSAN MENDUS, editor. *Justifying Toleration: Conceptual and Historical Perspectives*. New York: Cambridge University Press. 1988. Pp. vii, 260. \$39.50.

SUSAN MENDUS, Introduction. RICHARD TUCK, Scepticism and Toleration in the Seventeenth Century. ALAN RYAN, A More Tolerant Hobbes? JEREMY WALDRON, Locke: Toleration and the Rationality of Persecution. DAVID EDWARDS, Toleration and Mill's Liberty of Thought and

Discussion. NICHOLAS DENT, Rousseau and Respect for Others. D. D. RAPHAEL, The Intolerable. JOSEPH RAZ, Autonomy, Toleration, and the Harm Principle. MARGARET CANOVAN, Friendship, Truth, and Politics: Hannah Arendt and Toleration. G. W. SMITH, Dissent, Toleration, and Civil Rights in Communism. GRAEME DUNCAN and JOHN STREET, Liberalism, Marxism, and Tolerance. DAVID MILLER, Socialism and Toleration.

C. L. N. RUGGLES, editor. *Records in Stone: Papers in Memory of Alexander Thom*. New York: Cambridge University Press. 1988. Pp. xvii, 519. \$100.00.

ARCHIE THOM, A Personal Note about My Late Father, Alexander Thom. HANS MOTZ, A Personal Appreciation of Professor Alexander Thom. ARCHIE THOM, The Career and Publications of Alexander Thom. LESLEY FERGUSON, A Catalogue of the Alexander Thom Archive Held in the National Monuments Record of Scotland. ALEXANDER THOM and ARCHIE THOM, The Metrology and Geometry of Megalithic Man. CHRIS JENNINGS, Megalithic Landscapes. AUBREY BURL, "Without Sharp North . . ." Alexander Thom and the Great Stone Circles of Cumbria. EUAN MACKIE, Investigating the Prehistoric Solar Calendar. CLIVE RUGGLES, The Stone Alignments of Argyll and Mull: A Perspective on the Statistical Approach in Archaeoastronomy. JON PATRICK and PETER FREEMAN, A Cluster Analysis of Astronomical Orientations. RAY NORRIS, Megalithic Observatories in Britain: Real or Imagined? LESLIE MYATT, The Stone Rows of Northern Scotland. PIERRE-ROLAND GIOT, Stones in the Landscape of Brittany. DAVID FRASER, The Orientation of Visibility from the Chambered Cairns of Eday, Orkney. GRAHAM RITCHIE, The Ring of Brodgar, Orkney. RONALD CURTIS, The Geometry of Some Megalithic Rings. THADDEUS COWAN, Megalithic Compound Ring Geometry. ALAN DAVIS, The Metrology of Cup-and-Ring Carvings. MARGARET PONTING, Megalithic Callanish. ANTHONY AVENTI, The Thom Paradigm in the Americas: The Case of the Cross-Circle Designs. ED KRUPP, Light in the Temples.

R. R. DAVIES, editor. *The British Isles, 1100–1500: Comparisons, Contrasts, and Connections*. Edinburgh: John Donald; distributed by Humanities, Atlantic Highlands, N.J. 1988. Pp. xi, 159. \$55.00.

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PHILLIP N. BEBB and SHERRIN MARSHALL, editors. *The Process of Change in Early Modern Europe: Essays in Honor of Miriam Usher Chrisman*. Athens: Ohio University Press. 1988. Pp. vii, 218.

PHILLIP N. BEBB and SHERRIN MARSHALL, Preface: Miriam Usher Chrisman: In Appreciation. SHERRIN MARSHALL, Introduction: The Process of Change in Early Modern Europe: Urban Society, Intellectual Development, and Family Life. THOMAS A. BRADY, JR., The Reformation's Fate in America: A Reflection. JEAN ROTT, The Library of the Strasbourg Humanist Thomas Wolf, Senior (+1511). PHILLIP N. BEBB, Humanism and the Reformation: The Nürnberg *Sodalitas* Revisited. SUSAN C. KARANT-NUNN, What Was Preached in German Cities in the Early Years of the Reformation? *Wildwuchs* Versus Lutheran Unity. HANS R. GUGGISBERG, Sebastian Castellio and His Family. JEROME FRIEDMAN, Samuel Usque's Jewish-Marrano Nicodemite-Christian Apology of Divine Vengeance. R. POCHIA HSIA, Printing, Censorship, and Antisemitism in Reformation Germany. MARK U. EDWARDS, JR., Statistics on Sixteenth-Century Printing. ELLIS L. KNOX, The Lower Orders in Early Modern Augsburg. MERRY E. WIESNER, Paternalism in Practice: The Control of Servants and Prostitutes in Early Modern German Cities. LORNA JANE ABRAY, Joyful in Exile? French-Speaking Protestants in Sixteenth Century Strasbourg.

ROBERT V. SCHNUCKER, editor. *Calviniana: Ideas and Influence of Jean Calvin*. (Sixteenth Century Essays and Studies, number 10.) Kirksville, Mo.: Sixteenth Century Journal. 1988. Pp. 288. \$30.00.

ROBERT M. KINGDON, Introduction. I. JOHN HESSELINK, Law and Gospel or Gospel and Law? Calvin's Understanding of the Relationship. MERWYN S. JOHNSON, Calvin's Handling of the Third Use of the Law and Its Problems. W. FRED GRAHAM, Calvin and the Political Order: An Analysis of the Three Explanatory Studies. RICHARD C. GAMBLE, Calvin's Theological Method: Word and Spirit, A Case Study. CHRISTOPHER B. KAISER, Calvin's Understanding of Aristotelian Natural Philosophy: Its Extent and Possible Origin. DAVID FOXGROVER, The Humanity of Christ within Proper Limits. J. WAYNE BAKER, Calvin's Discipline and the Early Reformed Tradition: Bullinger and Calvin. CLAUDE-MARIE BALDWIN, Marriage in Calvin's Sermons. BRIAN G. ARMSTRONG, The Changing Face of French Protestantism: The Influence of Pierre Du Moulin. DAN G. DANNER, Calvin and Calvinism: The Career of William Whittingham. DONALD K. MCKIM, Some Aspects of Death and Dying in Puritanism. IAN HAZLETT, A Working Bibliography of Writings by John Knox. TIMOTHY GEORGE, John Calvin and Menno Simons: Reformation Perspectives on the Kingdom of God. BOBO NISCHAN, The Schools of Brandenburg and the "Second Reformation": Examples of Calvinist Learning and Propaganda. CHARMARIE J. BLAISDELL, Calvin's and Loyola's Letters to Women: Politics and Spiritual Counsel in the Sixteenth Century. JAMES TORRANCE, Interpreting the Word by the Light of Christ or the Light of Nature? Calvin, Calvinism, and Barth.

ERIC M. SIGSWORTH, editor. *In Search of Victorian Values: Aspects of Nineteenth-Century Thought and Society*. Manchester: Manchester University Press; distributed by St. Martin's, New York. 1988. Pp. v, 202. \$39.95.

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PHILIP M. TAYLOR, editor. *Britain and the Cinema in the Second World War*. (Proceedings of the Britain and the Cinema in the Second World War Conference, 1985.) New York: St. Martin's. 1988. Pp. x, 210. \$29.95.

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T. M. DEVINE and ROSALIND MITCHISON, editors. *People and Society in Scotland*. Volume 1, 1760-1830. Edinburgh: John Donald, for the Economic and Social History Society of Scotland; distributed by Humanities, Atlantic Highlands, N.J. 1988. Pp. xii, 316. \$35.00.

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ISON, The Poor Law. W. HAMISH FRASER, Patterns of Protest. TONY CLARKE and TONY DICKSON, The Birth of Class?

RAFAEL BAÑON MARTINEZ and THOMAS M. BARKER, editors. *Armed Forces and Society in Spain: Past and Present*. Boulder, Colo.: Social Science Monographs; distributed by Columbia University Press, New York. 1988. Pp. xi, 379. \$35.00.

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Documents and Bibliographies

The following collections of documents, bibliographies, and other similar works were received by the AHR between October 2 and February 14, 1989. Books that will be reviewed are not usually listed, but listing does not necessarily preclude subsequent review.

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Other Books Received

Books listed were received by the *AHR* between October 2 and February 14, 1989. Books that will be reviewed are not usually listed, but listing does not necessarily preclude subsequent review.

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- APPLEBAUM, RICHARD P. *Karl Marx*. (Masters of Social Theory, number 7.) Newbury Park, Calif.: Sage. 1988. Pp. 160. \$9.95.
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ARTICLES

TO THE EDITOR:

Alan E. Samuel's perceptive and penetrating article, "Philip and Alexander as Kings" (*AHR*, 93 [December 1988]: 1270–86), touches upon the interaction of early kingship with military leadership. My research interest has been in similar Germanic aspects, in particular, the role of Germanic war bands during the pagan and/or migratory period. In my work (see my last article for references, *Central European History*, September 1981), I could not help but note parallels to the ancient field, parallels I wish I had another lifetime to develop. My point is that the Germanic war bands were separate entities that played a significant role in tribal government as well as in the conquest and settlement during the Germanic expansion. These bands consisted of free, unmarried men (their leadership was not hereditary), and their leaders had priestly functions (oracle and sacrifice) with an autonomous status. Before going into battle, they swore by their deity to sacrifice every last enemy or be killed themselves (compare the Spartans at Thermopylae). When settling on conquered land, they would practice *Raubebe* (compare the rape of the Sabine women and perhaps Helen of Troy). They also served as a police force with powers to convict and execute without a hearing (the English *soc*; compare Spartan *krypteia*). The Germanic war bands lost their public role after settlement and conversion to Christianity. Samuel's indirect suggestion that their dynamism continued in the Carolingian kingship should be accepted as a significant con-

tribution. Perhaps the nature of kingship in Macedonia may be better understood if traced to the role of similar war bands of an earlier period.

ARNOLD H. PRICE

American Historical Association

Alan E. Samuel did not wish to respond.

THE EDITOR

TO THE EDITOR:

The one question that comes immediately to mind about Michael H. Kater's fascinating, richly researched, and illuminating article, "Forbidden Fruit? Jazz in the Third Reich" (*AHR*, 94 [February 1989]: 11–43), concerns its treatment of that most elusive and perhaps indefinable term—jazz. My *Webster's Collegiate* surely gets it wrong when it states under definition "a" that jazz must be characterized, among other things, by "varied orchestral coloring." For where does that leave a Lester Young solo on "These Foolish Things" or Louis Armstrong singing "Exactly Like You"? My *Oxford English Dictionary's* definition, "any syncopated dance music," surely sacrifices accuracy for succinctness, for it appears to include the corniest and squarest music imaginable, so long as it has the requisite beat, while excluding any number of Billie Holiday treatments of almost dirge-like ballads. The point is that, while it may be impossible to get it just right, there are many ways to get it wrong. I think Kater's usage is, in a number of instances, problematic.

Both jazz musicians and scholars of jazz would, I think, question the appropriateness of his article's very title. For his essay is not about jazz, as the term has been understood from Buddy Bolden's time, through Armstrong's, to Miles Davis's but about "jazz," fast or lively dance music or "what is often called jazz." If there is one musical ingredient that is essential in jazz, it is the distinctive quality of the sound(s) made by the musicians. As I write, I am listening to a radio broadcast of the

music of Roy Eldridge, who died on February 27. Eldridge—known as “Little Jazz”—transmuted banal pop tunes into jazz by his matchless way of infusing a note or series of notes with a feeling and pulse and tone color and “attack” that are somehow congruent with the tradition that goes back at least a century to the black musicians of New Orleans and its environs.

Armstrong said you know jazz when you hear it. Most of the orchestras Kater writes about did not play what Louis Armstrong or Leonard Feather or Hugues Panassié would call jazz.

The article speaks of “jazz titles.” The problem is that “Bugle Call Rag” or “Undecided” or other “jazz titles” are not jazz when played by square musicians. When Louis played anything, it turned into jazz. Just hear his or Chu Berry’s remarkable versions of Carmen Lombardo’s “Sweethearts on Parade”—a song that Guy Lombardo’s Royal Canadians played with the surpassing syrupiness that Carmen doubtless heard in his mind’s ear when he composed the song.

At several points, Kater writes that playing from arrangements is antithetical to jazz (pp. 24, 33). Tell that to Fletcher Henderson, Edgar Sampson, Jimmy Mundy, Claude Thornhill, and Mary Lou Williams, whose remarkable arrangements were swung beautifully by Benny Goodman’s fine jazz men and before that by the jazz orchestras of Henderson and Chick Webb. Duke Ellington’s band meticulously playing Billy Strayhorn’s haunting arrangements is playing jazz—the marvelous jazz that Johnny Hodges, Ben Webster, Cootie Williams, Duke, and their colleagues could play.

Nor does improvisation have quite the significance in jazz that Kater attributes to it. Anyone can improvise. What stands out in the improvisations of Armstrong, Coleman Hawkins, Charlie Parker, Jess Stacy, Bud Freeman, and other great jazz musicians is the jazz feeling that infuses the variations they play on a theme, as they enlist memory, intuition, mother wit, and virtuosic competence to create their “new songs.”

Finally, I fear that Kater may be romanticizing jazz, as when he calls it a “music redolent of liberty,” with a “strong tradition of . . . commitment to freedom and social justice” (pp. 13, 43). That jazz was for a while a music played only by blacks, the most victimized minority in American life, or that it does require its practitioners to play or sing with strong feeling does not quite represent what Kater suggests it does. Both the words and the music of Verdi’s marvelous duet for Don Carlos and Rodrigo, as they pledge to struggle for freedom for Flanders, indeed represent a “commitment to freedom and social justice.” Jazz has no political content, even when, as in Ellington’s

“Harlem Airshaft,” it is programmatic. That the Nazis hated it and that musical snobs had contempt for it does not make it what it is not.

EDWARD PESSEN
*Baruch College,
City University of New York*

MICHAEL H. KATER REPLIES:

Edward Pessen is right in suggesting that for a historian, or for any scholar, the proper definition of jazz is a difficult task. I would claim that defining jazz is as difficult as defining truth. For a musician, this is less of a problem because he does not have to resort to Webster’s dictionary to know immediately whether a jazz tune he and his group are playing “swings” or not. If it swings, it is jazz. During many years of playing in the post-bebop style (“mainstream jazz”) with a variety of professional groups, in Canada, Germany, and Austria, my co-musicians and I have come to accept this as an implicit definition of jazz, without losing many words about it. Often, jazz musicians do not like to talk, and, if you have any explaining to do, you might as well stop playing. (Now, if I have to start explaining why one piece swings and another one does not, I would have to write a musicological dissertation . . .)

I fail to understand why Pessen thinks that my article is not about jazz. Certainly, it is about that form of music then played and listened to by Englishmen, Frenchmen, Dutchmen, and Scandinavians, as well as Germans, all over Europe, even though it may have been a copy of the genuine article from America. Undoubtedly, some of it did not swing, and hence that was not jazz. But this could also have happened to the music being played contemporaneously in Kansas City, Philadelphia, or Chicago. The danceability of jazz was not then and is not now a criterion of the essence of the music; the 1930s jazz form called “swing” is still the best example of this. From the point of view of the musician, the sense of what he and his colleagues call “time,” the presence or absence of flat thirds, flat sevenths, or “flat fives,” the right perception of upbeats or downbeats, could make all the difference. Remember what Dexter Gordon’s character said in the movie “Round Midnight”: “And then they invented the flat five!” That, to him, completed the creation of the modern jazz idiom. Listening to German jazz players of the 1930s and 1940s and then to their American counterparts on records, I was able to find little distinction in the execution of their jazz tunes other than what I tried to describe in my article: it was mostly a matter of degree, not of substance.

Hence I agree with Pessen that jazz titles played by square musicians are not jazz. These square

musicians could be called Barnabas von Géczy, Will Glahé, or José Wolff in Germany, and Guy Lombardo, and even Sam Wooding and Paul Whiteman in the United States. It is just that in Germany you had more of these non-hip players than in the country of origin. But, as I have pointed out, pianist Fritz Schulze, reed-player Ernst Höllerhagen, and drummer Fritz Brock-sieper could all have held their own with any of their American colleagues stateside, if given a chance. Post-1945 German musicians such as Rolf Kühn and Jutta Hipp, who went to the United States, have proved this.

Playing from arrangements is something any good jazz musician should be able to do, but it does not constitute the essence of jazz. It is improvisation along pre-suggested melodic, harmonic, and rhythmic patterns that is the essence. Fletcher Henderson, Claude Thornhill, Mary Lou Williams were all able to improvise besides reading arrangements. And so was the Duke. (I thoroughly disagree with Pessen, who may not be a jazz player himself, when he categorically states that "anyone can improvise.") This was *not* the case with many German jazz musicians who could merely read from sheet music and did not even understand chord symbols, let alone the substitution of a new melodic line for the original one.

Finally, I may wish to plead guilty to romanticizing jazz when I maintain that it is "redolent of liberty." In itself, jazz can be a romantic music, and it is best played by free spirits who do not fit into dictatorial and totalitarian systems. Recently, why was the "Jazz Section" hounded down in Czechoslovakia? Because its members stood for creative (and other) freedom. Why was Benny Goodman endlessly cheered when his band played in Moscow in 1962? Certainly because his music swung, which it did, but also, and particularly, because it represented a kind of freedom that Russian society was not sharing at that time.

MICHAEL H. KATER
York University

REVIEWS OF BOOKS

TO THE EDITOR:

In her review of my book, *Sex and Power: The Rise of Women in America, Russia, Sweden and Italy* (AHR, 93 [December 1988]: 1289), Lois Banner complains that my book is too long: this she attributes to "weak editing." My book enjoyed strong editing from start to finish. Its length is my responsibility and mine alone. My book has an argument. I intended that that argument be conveyed, not

through superimposed theory but in its structure and in the copious detail of real history. In suggesting that my book not only should be much shorter but also include two more nations, Banner simply visualizes another kind of book altogether, one I had no intention of writing and did not write.

DONALD MEYER
Wesleyan University

Lois Banner did not wish to reply.

THE EDITOR

TO THE EDITOR:

Harry J. Ausmus seems to have philosophical objections to the notion that "naturalism" (or any other world view, for that matter) can be nonreligious. The unfortunate result of this refusal to discriminate between religious and nonreligious world views is that he does not engage the thesis of my book, *Explaining Religion: Criticism and Theory from Bodin to Freud* [AHR 93 (December 1988): 1293-94]. In that book, I try to give a plausible account of how naturalistic (nontheological, non-religious) explanations of religious phenomena emerged from about 1600 on, gathered momentum, and finally formed the critical core of an approach that is (in my view) paradigmatic for the study of religion in contrast to "theology," which, as I used the term, typically makes reference to transcendence in its explanations (explicit or implied) of religion.

We have a legitimate disagreement about how we use the word "religion," but it is not as though it has some fixed, transcendental meaning that Ausmus may impose on those who use it differently. My procedure, he says, would "limit the definition of *religio* to only certain institutions, to formalized theology, indeed to a restricted linguistic orbit." That is exactly the point: "religion" as an object of study *has* to be delimited in some way merely to get clear about our object of study, to distinguish it from other cultural artifacts, and to distinguish our discourse from the normative languages of theology, metaphysics, and religious philosophy. That is an important problem in the field right now.

If Ausmus is right, however, the task is hopeless; the study of religion is impossible to define because it is a study of everything. Marxism is religion; so are naturalism, historicism, and let us not forget secularism . . . You name it—*any* distinction between religious and nonreligious outlooks is spurious. Thus Ausmus characterizes the naturalism of Durkheim, Freud, and myself as "religious commitment," putting it in quotes so

that the reader does not even know if that phrase is mine or his (it is his). His review reminds me of some apologists for religion who, for whatever reason, seek to correct misguided souls (especially Marxists) who say that they are not religious.

A couple of other points: nowhere in the book do I myself try to explain religion, as the review claims, nor do I propose that my own historical approach itself constitutes a new paradigm for studying religion. I do not expect everyone to agree with my interpretation of this chapter in intellectual history, or with my definition of the domain of the study of religion, but it does not seem too much to expect a reviewer to describe my project correctly. Finally, it is not entirely appropriate for Ausmus to imply that I lack understanding of secularization. I may—but secularization is the subject of *his* book, not mine.

J. SAMUEL PREUS
Indiana University

HARRY J. AUSMUS REPLIES:

Anyone with a cursory knowledge of my works will laugh uproariously at the suggestion that I am some kind of an apologist for religion. Rather, I am merely in fundamental agreement with the Vichean idea that the order of ideas follows the order of institutions. This means that, when the church is hegemonous, theological language will be dominant. When the state is hegemonous, socio-political and economic language, or what Vico called "philosophy," will be dominant. The historical process whereby theological language becomes interchangeable with socio-political and economic language is the intellectual side of the secularization process. Both are religious (*religio*, *religare*) for Vico. And, as I have shown elsewhere, both are nihilistic.

When Mikhail Gorbachev says that the religious spirit in man will never be destroyed, are we to dismiss this view as merely a Machiavellian political ruse? When Martin Heidegger, with hope-filled faith, patiently awaits the "new revelation" of Being, are we to dismiss this statement as the ramblings of a Black Forest recluse? When Friedrich Nietzsche asserts that history has become a disguised theology, are we to dismiss this claim as the product of a syphilitic mind? When Karl Marx seeks to establish a "perfect Christian society," are we to dismiss this proposal as the

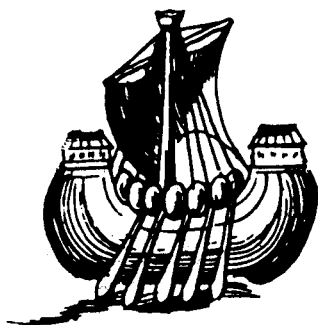
result of an atheist with a guilty conscience? When Friedrich Schelling proclaims that atheism and agnosticism are merely varied forms of Protestantism, are we to dismiss this assertion as merely one person's opinion? When Hegel announces that he has translated the language of theology into that of philosophy, are we to dismiss this basic concept as the fuzzy thinking of an abstruse metaphysician? These assertions can only be understood as religious assertions, however secularized they may be. The meaning of the word religion (*religio*) is far broader than some may assume, even so broad that the early Hebrews maintained that human existence itself was religious.

Indeed, given the hierarchical nature of our language, religion is a far more generic term than naturalism. To attempt a detached naturalistic approach to religion is somewhat on the order of an apple attempting a detached analysis of fruit. It can do so only if it acknowledges that it too is a fruit, in which case it is not detached. Or, the apple can arbitrarily assume that it is on a par with fruit, in which case, what is specific becomes, arbitrarily, generic. Likewise, one can engage in a naturalistic analysis of the institution of the church, the priesthood, the monastic system, sects, and denominations, and can even engage in a critique of theology from the perspective of sociology, psychology, or metaphysics. For example, naturalism, such as represented by Baron de Holbach, excoriated a fat church but in behalf of a fat state. It stopped believing in the language of the church and transferred that belief to the language of the state. A nontheological *Weltanschauung* is blatantly possible, although a religious enterprise. A nonreligious *Weltanschauung* is a blatant *contradictio in adjecto*.

I placed the phrase "religious commitment" in quotation marks for emphasis. And J. Samuel Preus is quite correct in saying that the phrase was not his. I should have underlined it. Nevertheless, I am in full agreement with Preus that an understanding of *religio* makes his kind of task "hopeless," even for those secularized fundamentalists who adamantly believe (*nota bene*) in the dogma of naturalism. Still, it is refreshing to see a scholar attempt to defend his position with such religious fervor.

HARRY J. AUSMUS
Southern Connecticut State University

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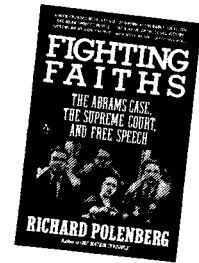
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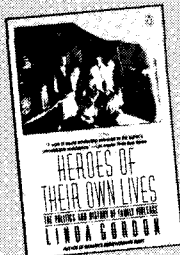
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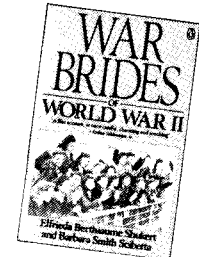
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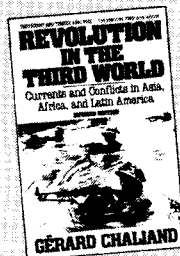


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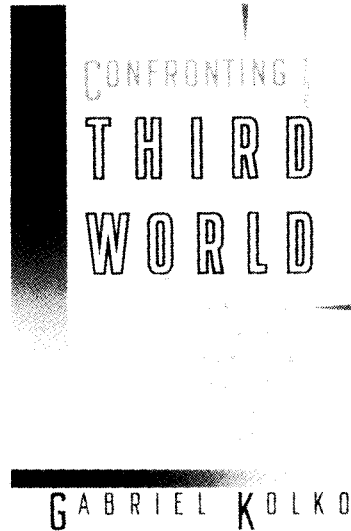
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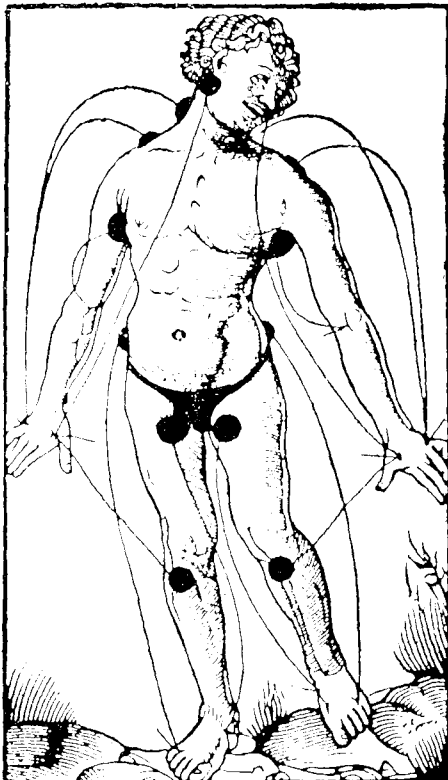
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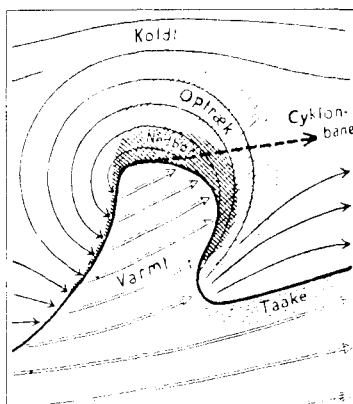
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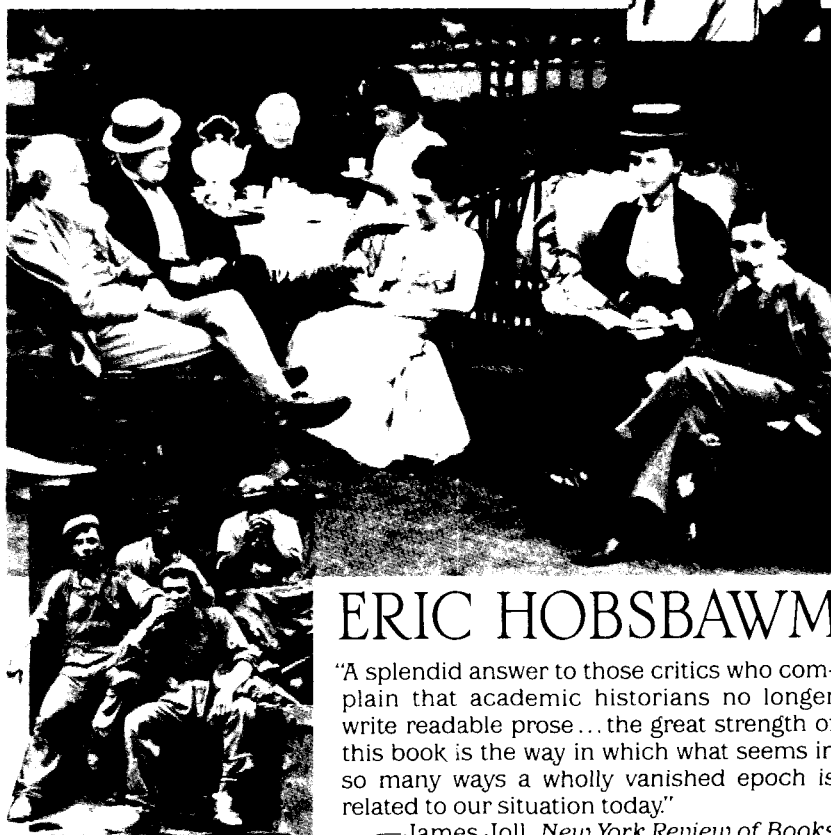
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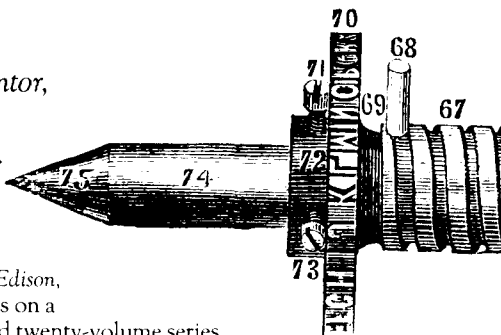
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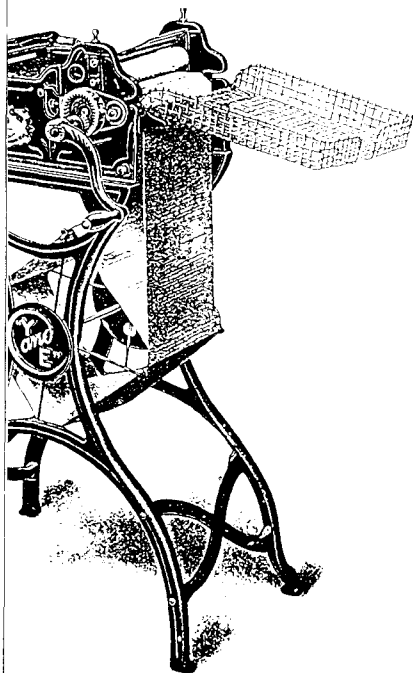
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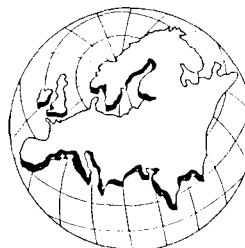
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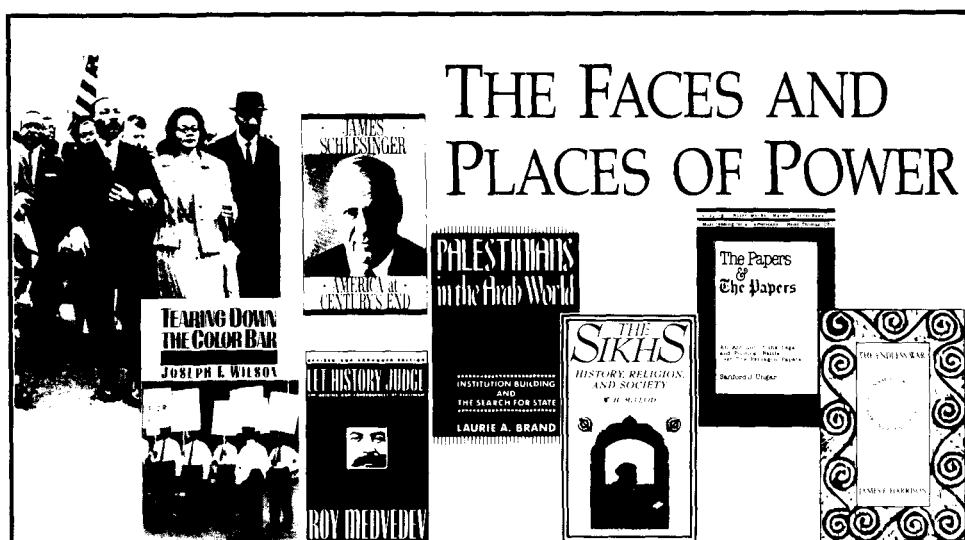
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